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ORIGINAL LECTURES.

CLINICAL LECTURE

ON A CASE OF DIABETES MELLITUS.

Delivered at the Philadelphia Hospital

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Reported by W. NORTON WHITNEY, M.D.

THE case presented to-day is one exhibiting in a well-marked degree some of the characteristic symptoms of diabetes mellitus. Its history is as follows.

J. A., æt. 36, born in Ireland; father living and in good health; mother dead. He has five brothers and one sister living and healthy. He worked on a farm until 16 years of age, when he came to this country, where he was employed in an oil-cloth factory for six years, since when he has been variously engaged in such occupations as bar-tender, porter, car-conductor, etc. He had a chancre nearly four years ago, and since then his health has not been good. About fourteen months ago he noticed that he had an unquenchable thirst, with increased appetite, notwithstanding which he lost flesh. He complained also of stiffness and numbness in his feet. The urine was greatly increased in quantity, clear, and light in color. He was forced to rise four or five times during the night to urinate. Upon admission—February 2, 1881—he was passing thirty pints of urine in twenty-four hours. During the two months following the date of his admission to the hospital his urine averaged about twenty-five pints per day; but at the end of that time it was reduced from thirty to nineteen pints in twenty-four hours, under a dietetic treatment. From this time until he came under my care we have no records of any special treatment, and he used the usual mixed diet of the hospital. On November 4 he passed 18.5 pints of urine, with a specific gravity of 1032, and containing 18.5 ounces of sugar, in twenty-four hours. On November 5 he was ordered salicylate of sodium, gr. xx, four times a day. On November 11 the quantity of urine was 31.5 pints (sp. gr. 1032, and sugar 32.5 ounces) in the twenty-four hours. November 26.—Urine, 51 pints; sugar, 32.8 ounces. He weighed on this day 121.5 pounds. The salicylate of sodium was discontinued and codeia (gr. ½ t. d.) ordered. On November 27 he passed 50 pints; November 28, 41 pints; November 29, 41 pints; November 30, 40 pints, containing sugar 32 ounces. Codeia increased to gr. 1 t. d. December 1, 37.5 pints; December 2, 31.5 pints; weight, 124.5 pounds,—a gain of three pounds since November 26. December 3, 34 pints. Increased codeia to gr. 1½ t. d. December 4, 34 pints; December 5, 21½ pints; December 6, 30½ pints; December 7, 27½ pints; December 8, 21¾ pints; December 9, 17¾ pints.

It will be noted from the history that the patient had a chancre nearly four years ago, It has, of course, no necessary relation to his present disease; but he says that succeeding the chancre he had a bubo which stubbornly resisted treatment for eighteen months. During this period he became very much run down, and soon after—that is, fourteen months ago—the symptoms narrated made their appearance. As stated in the history, he came under my care on November 4. He was then passing eighteen and one-half pints of urine in twenty-four hours, of a specific gravity of 1032, and containing sugar in abundance. Hunger and extreme thirst were also noticeable symptoms. An obstinate ulcer upon one of the toes was present, which had ensued upon so simple a treatment as painting the toe with iodine. This is a significant symptom, one which is not unusual, and indicates an advanced degree of malnutrition. For the same reason surgical operations are often unsuccessful and sometimes fatal. Gangrene from trivial causes is a result of such conditions.

The symptoms named are of themselves suggestive, but others are also present. Dryness of the mouth and skin, and absence of perspiration, may be mentioned. Itching of the skin is a more uncommon symptom of the disease. This symptom is often annoying in the vicinity of the labia in females, and of the meatus urinarius of males, when it is ascribed to the large quantity of sugar-saturated water passing over them. In advanced stages the lungs are often affected, and a cough is frequently the symptom of the beginning of cheesy phthisis, which is frequently the immediate cause of death. Cataract also occurs in the later stages. Extreme muscular weakness, and tottering gait, are conspicuous symptoms. The blood as well as the urine contains sugar, and sometimes, in the latter stages, its derivatives alcohol and acetone. The latter contributes an acetous or vinous odor to the breath, which is present in our patient. The blood-corpuscles are also said by

Griesinger to be diminished in number,—an observation recently confirmed by Dr. F. P. Henry, of Philadelphia. Lipæmia, or an excess of fat, in a state of molecular subdivision, in the blood, is a symptom sometimes met with.

Such is a brief picture of the most striking symptoms of diabetes, not all of which are presented by our patient, although the thirst, dryness of skin, abundant urine, enormous appetite, accompanied by wasting and loss of strength, are here strongly suggestive of diabetes. They can hardly be said to be sufficiently diagnostic in the face of the existence of another form of polyuria in which all of the latter symptoms may be present while sugar is absent from the urine. This condition is *diabetes insipidus*. The diagnosis is made by a further study of the urine. In both it is excessive; but in the saccharine diabetes it is of high specific gravity and contains sugar, while in the diabetes insipidus it is of low specific gravity and contains no sugar.

Let us therefore study the urine together. Observe its clear, almost colorless appearance; its specific gravity, 1032, not as high as it has been, but still much above the normal. In a case of diabetes insipidus shown you a few weeks ago it was 1002. Let us go further and test it for sugar. The best qualitative tests for sugar are the copper tests and the fermentation test, but the former enable us to detect smaller quantities of sugar. Of the copper tests, the solution known as Fehling's is more delicate and less liable to mislead in its results. But often the Fehling's solution is not at hand or not obtainable when the reagents for Trommer's are. The latter is best used by adding to a small quantity of urine in a test-tube half its bulk of solution of potash or soda, *then just enough solution of sulphate of copper to give the mixture a distinct blue tinge.* If sugar is present, the precipitate of protoxide of copper which takes place the moment the copper falls into the mixture is redissolved on agitation: so that such solution is presumptive evidence of the presence of sugar, while it is, however, not conclusive. If now the mixture be boiled, if sugar is present the protoxide is reduced and a yellow hydrated suboxide of copper is thrown down, which is soon dehydrated and becomes red.

As other substances besides sugar may reduce the copper or mask the precipitate, their presence sometimes becomes a source of error. Uric acid, for example, may reduce the protoxide of copper, while the presence of ammonium chloride and creatinine may prevent the precipitation of the red suboxide. The amount of sugar thus masked is usually small and not often of much clinical importance. A precipitate, and not a simple decoloration, is an essential criterion of the presence of sugar, while the gray precipitate of earthy phosphates which always takes place is scarcely likely to be mistaken for that of red suboxide of copper. *The addition of too much of the copper solution must also be avoided,* because by such a course a yellow precipitate, on boiling, may be obtained from any urine. Fehling's test depends upon the same principles as Trommer's, and is consequently subject to the same errors, but it is rather more delicate, probably because a more thorough solution of the copper is obtained by the tartaric acid it contains. I have said that if we add liquor potassæ to a solution of cupric sulphate, the protoxide of copper is precipitated, which is again dissolved if sugar is present. But to produce a permanent solution sugar is evidently unsuitable. For this purpose, therefore, some other agent is necessary. Such an agent is tartaric acid or tartrate of potassium, as employed in this test.*

The principal objection to Fehling's solution is its liability to alter when kept for any length of time. On using it, therefore, it should always be heated, and for qualitative testing it is preferable first to dilute it by about four times its bulk of distilled water. If the suboxide is not precipitated by boiling, the solution is good, and a drop of the suspected urine is allowed to fall into the boiling solution. If sugar is present in large amount, a single drop of the urine will often throw down a dense orange-red precipitate. At other times a larger quantity of urine must be added; but if no suboxide is thrown down by the time an equal bulk of urine is added, the boiling being repeated, it may be considered for clinical purposes that sugar is absent.

* The following is the formula for Fehling's solution: Dissolve 34.652 grammes cupric sulphate in 200 grammes distilled water, and 173 grammes of neutral tartrate of sodium in 500 to 600 grammes of solution of caustic potash (sp. gr. 1.12); add the copper solution to the soda solution, and dilute to 1 litre. Ten cubic centimetres of this solution correspond to .05 gramme of sugar.

The fermentation test depends upon the power of yeast to decompose sugar into alcohol and carbonic acid. To, say, four ounces of the suspected urine of which the specific gravity has been taken, a little yeast is added. The mixture is kept at a temperature of 100° F. for at least twelve hours. The specific gravity is again taken; and if it is less than before fermentation, sugar has been present. This test has also the advantage of being quantitative; for Dr. Roberts, of Manchester, has shown that the difference in specific gravity before and after fermentation indicates the number of grains of sugar per fluidounce.

By means of this last test we have found that our patient, a few days ago, was passing twenty-four grains to the fluidounce, or thirty ounces of sugar in twenty-four hours. This is a very large quantity, and more than the usual secretion, even in severe cases. The largest quantity on record is, I believe, fifty ounces in twenty-four hours; but the average is from half an ounce to eight ounces in urine containing from five to thirty grains to the ounce. The quantity of the urine varies considerably, and has been said to reach seventy pounds in a day.

A few words on the *pathology* of the disease in question. It is well known that the liver, from the glucose prepared for it in the intestinal digestion of starchy and saccharine foods, forms glycogen, which it stores up in its cells for future use. This glycogen is reconverted into glucose slowly and gradually as the wants of the economy require it. Now, it is possible that, in consequence of some derangement of the function of the liver, the glucose may pass directly through it without being first converted into glycogen; or the glycogen, once formed, may be converted into glucose more rapidly than it can be consumed in the blood, so that it accumulates there and is eliminated by the kidneys. The former is the view of Pavy, the latter that of Claude Bernard. It is not impossible that glycosuria may result in both ways.

It has been further experimentally ascertained that irritation of a certain point in the floor of the fourth ventricle (the nib of the *calamus scriptorius*) is followed by glycosuria. It is found that this is accompanied by a dilatation of the blood-vessels of the liver, which allow the blood to flow through them with more than usual rapidity,—a rate much too great to permit the normal conversion of glucose into glycogen and glycogen into glucose. But there are other ways also in which we may conceive sugar to enter the blood in excess and reappear in the urine. The simplest of these is over-ingestion of sugar-producing food or sugar itself. Such a cause would be a diet of pure vegetable and starch foods. From such causes as these it is not unlikely that mild cases of diabetes originate; and it is such cases that are readily cured by cutting off starchy and saccharine foods.

From the fact that the nervous irritation I have mentioned produces glycosuria, it is held by some that a nervous lesion is always at the bottom of diabetes mellitus: in fact, it is defined by Dickinson as a nervous disease. This view I cannot accept; for, apart from the fact that many cases of true diabetes terminate without the slightest trace of nervous lesion, most cases of glycosuria which are directly traceable to nervous lesions fail to exhibit any of the other symptoms of diabetes; and simple glycosuria is not diabetes.

When we seek for a *morbid anatomy* of diabetes, none that may be considered essential meets us. Dr. Dickinson describes a peculiar "cribriform condition" of the nerve-centres, which is apparently the result of hemorrhagic extravasations about the small blood-vessels, with subsequent destruction and absorption of the nervous matter in the neighborhood. These changes, he says, are easily visible to the naked eye; but as the same condition has been discovered in other diseases, and has been found wanting in many cases of diabetes, we can hardly claim that it is its true cause. It is not impossible that it is a secondary result of the saturation of the vessels with sugar from the saccharine blood by which their walls are bathed. The liver is generally enlarged, sometimes to two or three times its normal size, and its cells show a disposition to fuse together and to strike a red color with iodine. Rindfleisch describes the changes as most marked in the peripheral zone supplied by the portal vein, while that of the hepatic artery is fatty, and the inner zone, or that of the hepatic vein, is nearly normal.

The kidneys, in advanced cases, sometimes show the results of long-continued hyperæmia by the presence of parenchymatous nephritis,—results which are secondary only. The pancreas is surprisingly often the seat of various disease, but no

essential relation has been shown to exist between its lesions and diabetes.

The *prognosis* of diabetes mellitus is generally considered unfavorable; but I am certain that the gravity of the average case is exaggerated by such a statement. Many cases are curable, and there are few which are not amenable to treatment; so that, if taken in time, the disease may be rendered harmless so long as the prescribed treatment is carried out. A few cases only are beyond relief when they come under the notice of the physician, and the unfavorable course of some is the result of an unwillingness of the patient to submit to treatment. The prognosis is most unfavorable in young patients under twenty. Where there is serious nervous lesion the prognosis depends upon that of the nervous disease itself.

Treatment.—While we cannot always reason from pathology to successful treatment, the relations between the two are so evident that I make no apology for having detained you with this discussion. Having learned that the starchy and saccharine foods pass through the body in diabetes without being assimilated, it is evident that they can serve no useful purpose as food. Much better, therefore, withhold them, and substitute such foods as can be assimilated and which may therefore contribute to the strength of the patient. Such foods, at least in the early stages, are fatty and albuminous substances. But the effect of such restriction seems, in some cases at least, to do more than merely supply an assimilable diet. Whether by furnishing rest and encouraging the natural reparative tendency of the organs involved, or in some other inexplicable manner, the more or less prolonged use of such a diet is sometimes followed by complete recovery. The details of a suitable non-amylaceous diet will be found in the text-books; but I will mention to you here, as admissible, beef, mutton, fish, oysters, game, eggs, milk, butter, non-saccharine wines. Nor need vegetables be entirely eschewed. The green vegetables, such as spinach, green cabbage, cauliflower, Brussels sprouts, string beans, water-cress, etc., may all be used in moderation, subject, however, to such variations as the peculiarities of the case may demand. To this may be added bread made of gluten flour, now readily obtainable and tolerably free from starch. Without going more minutely into the matter of diabetic diet, I would state that the variety of admissible articles is now so great that the monotony formerly so much complained of is no longer in the way of those who have the means to provide the variety permitted.

As a form of dietetic treatment may be mentioned the *skimmed-milk* treatment suggested by Dr. Donkin and very enthusiastically defended by him. At first, and indeed still, greatly opposed by many English physicians, the value of this mode of treatment is being more and more acknowledged as cases are reported of recovery under its use; and where it is possible and willingly submitted to by the patient, I now usually begin treatment with it, and, if the sugar disappears under its use, resort to no other. One of the greatest inconveniences in its use is the enormous amount of fluid which requires to be ingested in order to maintain the weight of the body at its standard, eight to fourteen pints *per diem* being sometimes required to do this. This is, however, diminished by taking part of the milk in the shape of curd. One-half or more of the skim-milk may be used in this way, and thus the ingestion of a large amount of water obviated. Where so much water is introduced, of course a proportionate amount must be excreted, which is always, however, somewhat less than that ingested, the lungs, skin, and fæces removing a part of the water. Of skim-milk ninety per cent. is water; so that this ninety per cent. must be excreted by the skin, lungs, bowels, and kidneys. It was observed by Dr. Donkin that the amount of urine secreted amounted, on an average, to two pints less than the fluid ingested; and in a patient now under my observation, who is drinking eight pints and using the curd from two pints, this proportion is maintained with remarkable constancy.* But it is evident that while this ratio of urine secreted to milk ingested is maintained for a given quantity as that ingested by the patient alluded to, the ratio could not be maintained for a smaller or larger quantity ingested.

You will probably infer from what I have said that I deem the dietetic plan of treatment more valuable than any other. At the same time I do not wish to be considered as altogether repudiating the medi-

* In the case referred to, the effect of the skim-milk diet is promptly to remove all traces of sugar when the urine is tested in the most crucial manner: while the addition of even gluten bread to the diet was followed by the return of sugar in the urine.

cinal. There are undoubtedly drugs which diminish the elimination of sugar, although I should be sorry to have to rely upon any one to the exclusion of diet. Among the most efficient in my hands has been the *fluid extract of ergot* in doses of from twenty drops increased to a fluidrachm three and even four times a day. I have found it, however, totally inefficient in some cases, where I have given as much as four drachms of the fluid extract in a day. It may be increased, if the stomach bears it, to two drachms three times a day; but more than this I should not care to give. *Opium* is a remedy to which was early ascribed efficiency in the treatment of diabetes; but its constipating tendencies contra-indicate its use, and clinicians have been searching among its alkaloids for the element producing the desired effect. There is reason to believe that *codeia* is at least one to which the benefit may be ascribed. It was long ago tried for this purpose, with only partially satisfactory results; but recently its use has been revived, with encouraging results, and I determined to try it in the present case. Unfortunately, our hospital cannot furnish the required conditions of a dietetic treatment. On the other hand, this very circumstance makes it possible to determine the true value of drugs independent of dietetic treatment; and this is certainly the only way to test them. Accordingly, after a trial of salicylate of sodium,—a remedy recently suggested, which I gave in doses of twenty grains four times a day for three weeks, without any result,—on November 26, on which he passed fifty-one pints of urine in the twenty-four hours, containing two pounds of sugar, I placed him upon codeia, —half a grain three times a day; increased it to one grain three times a day on the 30th; to one and one-half grains on December 3. On the day after the codeia was commenced he passed fifty ounces of urine; on the next day, thirty-seven and one-half ounces; and from that date the quantity of urine steadily diminished, until it reached, on December 9, seventeen pints and three-quarters. Corresponding to this diminished secretion, he tells us that his thirst is much less marked, and he drinks much less water. Indeed, he says he feels altogether better. He is constipated, but not more so than he was before taking the remedy; but no narcotic effect has resulted.

It is evident, therefore, that the use of the remedy in this case has been attended by an improvement of symptoms. I do not contend that we dare infer from the results reached at this stage of the present case that the remedy is an efficient one. I merely present it to you as another added to the cases already reported under this treatment. On the other hand, had the remedy failed, it would have been equally unfair to declare the remedy useless; for the case is a very bad one, and the quantity of urine and sugar secreted in the twenty-four hours is enormous; so that it is not likely that by any measures we can do more than palliate.

The quantity administered in this case is much less than is sometimes given. Dr. Pavy first recommended it, and gave as much as ten grains three times a day. Dr. Brunton recommends it in doses of one-quarter to one-half a grain three times a day at first, the doses being increased until the sugar disappears or drowsiness supervenes. Dr. Cavafy has given fifteen grains three times a day with satisfactory results. Dr. R. Shingleton Smith* considers that all other treatment, including dieting, is inferior to codeia. My experience with the drug has not been large enough to justify a decided opinion on the comparative value of codeia and diet, but, with such experience as I have had, I should be afraid to rely upon it to the exclusion of diet. I shall, however, continue the trial.

ORIGINAL COMMUNICATIONS.

SOME COMMENTS ON THE PAPER "THE ETIOLOGY OF TUMORS."†

Read before the Pathological Society of Philadelphia, April 28, 1881,

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IN the paper which is the subject of discussion this evening I endeavored to prove the proposition, viz., that all *primary* tumors, save the purely congenital neoplasms, are direct products of the inflammatory process.

A certain class of tumors are admitted by

* Philadelphia Medical Times, December 19, 1881, p. 127, from British Medical Journal, vol. ii., 1881, p. 474.

† The paper has been published in full in pamphlet form by order of the Society. An abstract of it, entitled "The Inflammatory Origin of Tumors," appeared first in Seguin's "Archives of Medicine," October, 1881.

several pathologists to be due to inflammation; I ascribe this cause to nearly all tumors. Again, those pathologists regard inflammation only as an exciting cause, provided there is a predisposition to tumor-formation. I am inclined to regard the inflammatory process as the factor which creates this predisposition, and hence consider inflammation as a direct predisposing cause for all true tumors. This is the difference between the view held by the real authorities of the inflammatory theory—S. D. Gross, Virchow, and Samuel—and the view which I advocate.

The idea of an inflammatory origin of tumors begins of late to gain more and more ground among the working pathologists. Several of the true tumors are nearly generally admitted to be due to inflammatory causes, and, although no one expresses himself decidedly upon the subject, I do believe that all will ultimately return to the view which the fathers of pathology originally held.

My studies on the etiology of tumors are by far not completed: still, I bring the work forward in its present state in order to get the full benefit of the criticism. I want advice and co-operation; I desire to learn whether the new facts which I obtained by microscopic and other studies may admit of an interpretation different from that which I gave them. At any rate, I consider my work only an attempt at the solution of the question of the etiology of tumors, a question so much neglected and which imperatively demands active work and not hypotheses.

Before I enter into a review of my arguments I desire here to call attention and to define more closely the purely congenital anomalies called "tumors," for which I am unable to prove an inflammatory cause.

The question first arises, what is a tumor and what is not a tumor?

In the sense of Virchow, any circumscribed elevation over a given surface, or any excessive enlargement, is considered a tumor. The products of specific inflammation, such as tubercle, gumma, glanders, lupus, and lepra, also the cysts and most of the monstrosities and the hypertrophies, would consequently belong here.

I consider the following neoplasms which are composed of new-formed or overgrown tissues as *true* tumors:

Fibroma.
Lipoma.
Chondroma.
Osteoma.
Leio-myoma.
Myxoma.
Lymphoma.
Sarcoma.
Glioma.
Papilloma.
Simple epithelioma (as represented by corns, horns, onychoma, etc.).
Carcinoma.
Tyroma (tubercular tumor).
Gumma.
Lupus, lepra, and glanders.

The following congenital neoplasms I consider as simple anomalies or *false* tumors:

Angeioma.
Lymphangeioma.
Some keloids and other nævi.
Rhabdo-myoma.
Adenoma.
Dermoids and
Other cysts.

All these last-named neoplasms should be excluded from the tumors. I suggest this, not because I cannot prove their cause directly to inflammation, but because they are simple anomalies or malformations, just like a supernumerary finger. Nobody can acquire any of them except the ordinary cyst. The individual must be born with them. Cohnheim calls them properly "monstrosities."

It is, however, possible that even here inflammation is concerned to some extent. Smallpox and syphilis, which are inflammations, are known to affect the fœtus in utero. Why, then, could we not have tumors as pre-natal inflammatory products? Still, I do not want to base my arguments on hypotheses. The fact is that children are born with large or small masses of any one of the above-mentioned congenital neoplasms.

Objection might be raised to the inclusion of adenoma in this category; but I have here reference mainly to the heterotopic adenoma, the perfect homologue of the mammæ, which grows prominent only at puberty, simultaneous with them, and governed in time and grows by the same laws. The glandular acini which gave rise to the adenoma and those from which the mammary glands started were both deposited in the fœtus, and in both were dormant up to puberty, when they developed to structures perfectly alike, the dif-

ference being only that the one has its physiological purpose and location, and the other not. The histological distinction between them as given by authors I was not able to see, after having examined *every* part of the structure. The homotopic adenomata, as occurring in connection with glands, are simple hypertrophies of any one of the racemose glands, or of a part of one.

The dermoid cysts are the best representatives of this group of anomalies. It is well established that they are simple local invaginations and misplacements of mainly epiblastic formations during early fœtal life. Certain parts of organs, such as skin, hairs, glands, teeth, etc., which usually are represented in these anomalies, proceed to full development and size and no further. There is nothing pathological in these structures except the location, unless combined with other new formation.

Ordinary cystic formations are also frequently met with in the fœtus, although many cysts are acquired in later life by the agency of various pathological factors, including inflammation. Frequently tumors are the seat of cystic formation due to degeneration or softening in their interior. The formation of cysts is nearly always a passive process. Many arise from mechanical obstruction of outlets of glands, or from exudation of liquid into closed cavities. None of them has anything in common with tumors except the tumefaction.

Angeioma and lymphangeioma and the keloids are exclusively congenital formations; they even seldom present themselves as tumefactions, and only then if subject to cavernous change and combination with other lesions. There is no reason why these anomalies should be classed with the tumors.

The same may be said concerning rhabdo-myoma, the strictly congenital rare new growth, made up of misplaced striated muscular tissue.

It is in these congenital neoplasms alone that an inflammatory origin is not clearly evident.

OTHER VIEWS.

Without entering into details, I will at this point enumerate the other theories on the etiology of tumors.*

Those theories, although held by high authorities, and ingenious as they are, hardly go beyond the level of pure hypothesis. Hypotheses and speculations are easily disposed of by facts like those presented in favor of an inflammatory origin of tumors.

No tumor has ever been *proven* to have originated *spontaneously*, or to be produced by a certain *dyscrasia* of the blood, or by *nervous influences*. We are not more justified in applying this or that pet hypothesis for the etiology of tumors without proving it, than to declare a house to have arisen through the instrumentality of mysterious forces because we do not know who built it, and do not care to inquire by whom and how it was built.

The evolution and involution of tissues as conditioned by age, referred to by Thiersch and Rindfleisch, and best explained by Dr. Ch. B. Nancrede (in his highly suggestive communication to the Pathological Society, 1876), are regarded as important predisposing factors in tumor-formation, which at the same time decide the variety of tumors.

There can be no doubt that evolution and involution of the tissues influence the kind of tumor-formation; but I do not believe that these conditions in themselves predispose particularly to tumors, even in the presence of an over-supply of blood. We want certain changes in the integrity of the tissue (to be referred to later), and these can be brought about by the inflammatory process alone,—by nothing else.

Due credit must also be given to Cohnheim for his embryonal theory of tumor-formation. Cohnheim uses the well-established congenital derivation of the dermoids, rhabdo-myoma, angeioma, etc., as a basis for his hypothesis, and jumps at once to the conclusion that all tumors are congenital and of embryonal origin. Some new formations which he admits to be of inflammatory origin—viz., gumma, tubercle, lupus, neuroma, osteophytes, etc.—he excludes from the category of true tumors.

But how great a reduction in number will Prof. Cohnheim's list of true tumors experience should it be proven that all tumors are of inflammatory origin save those few congenital formations which I suggest to exclude from the list of true tumors!

Epstein (*Zeitschr. f. Heilkunde*, i., 1880) believes to have found anatomical proof

* In my monograph, "The Etiology of Tumors," I gave all these views in full.

for Cohnheim's hypothesis. He observed epithelial pearls in the mucous membrane of the gums, tongue, and genitals of newborn infants, and regards them as the famous supernumerary embryonic collection of cells. This, I think, is a great error. It has been shown by several observers that wherever squamous stratified epithelium exists epithelial pearls may be found,—viz., in the epidermis and in all epiblastic mucous membranes. I believe that the arrangement of epithelium into pearls is always a sign of retrograde change, and, as well as the arrangement of any kind of cells into nodes, signifies usually an antemortem act of cells, and not "dormant supernumerary embryonal collections."

To find further proofs for the embryonal theory of Cohnheim, his pupils made extensive experiments. They succeeded; and the successful results, which were supposed to give a firm basis to the embryonal theory, were announced to the world in the renowned *Archives of Virchow*.

Unfortunately, however, for the Leipsic laboratory, the trifling efforts of American workers in experimental pathology gave results entirely opposed to those obtained by Cohnheim's pupils, and have probably forever demolished the beautiful embryonal theory of the etiology of tumors, as will be shown farther on.

EXPERIMENTS.

Allow me now to reflect one moment upon the results of the experiments made with the object of ascertaining the cause of tumors. I will review here only the main points of interest; the details are given in my monograph.

So far only little success was obtained in this line by experiments. Still, this much can be ascertained from them:

First, that tumor scannot be inoculated by virtue of any infective or specific properties; and

Second, that small living particles of tumors can be successfully transplanted from man to animals, and upon transplantation may continue to grow.

No one of the experimenters really succeeded to "inoculate" a tumor with tumor-juices except in a few very doubtful instances.

The juices as occurring in malignant tumors are always the products of degeneration of the tissue or cells composing them. The cells suspended in that juice are dead, having undergone fatty or some other degeneration, and this is the reason that injection with juices fails. If particles of living tumor-tissue happen to be suspended in that juice, "inoculation" might succeed, but not with mere pure tumor-juice. In the few apparently successful inoculations with juices, particles of perfect tumor-tissue undoubtedly were injected together with them.

The notion of a specific tumor-virus held by some of the highest authorities is thus fully disposed of.

There are recorded a number of successful transplantations of tumors: *i.e.*, small fragments of tumors when put into the subcutaneous tissue of animals grew and enlarged in size as long as observed, if conditions were favorable.

This, however, does not prove anything for the etiology of primary tumors. It has been shown that if a cock's spur be transplanted from the leg to the comb it will often grow excessively; another most perfect parallel to transplantations of tumors we have in skin-grafting and in plastic surgery.

Other observers, again, impressed with Cohnheim's idea that tumors arise only from misplaced (heterotopic) cells or tissues, experimented as follows:

1st. Particles of tissues taken from *adult* animals were introduced into the circulation and into the interior of organs, but they failed to grow and were ultimately absorbed.

2d. Fœtal tissues (particles of embryonal cartilage, etc.) were similarly transplanted, and they grew and developed to moderate-sized tissue-masses (tumors).

Through these results Cohnheim's proposition that tumors arise only from misplaced *embryonal* cells was regarded as proven.

But this was too hasty a conclusion, and it appears also that those experiments were conducted very carelessly, as the results could not be confirmed.

The exhaustive experiments which Henry Wile made in the pathological laboratory of the University of Pennsylvania (partly quoted in my monograph and partly not published yet) positively prove that transplanted *adult* tissues grow as well as fœtal ones, and never became absorbed in carefully-executed experiments. Thus the much-dwelt-upon proofs for the embryonal hypothesis of Cohnheim are gone.

INFLAMMATION AS THE SUPREME CAUSE.

Allow me now to review the arguments in favor of, and the proofs for, an inflammatory origin of tumors, as brought forward in my paper. These are of three kinds: 1st, proofs by analogy; 2d, clinical and statistical proofs; and 3d, microscopical proofs.

1. *The close analogy of tumors and inflammatory products is strongly in favor of our proposition.*

Careful study and comparison have shown that no line of distinction can be drawn between true tumors and chronic inflammatory products; in fact, many of the latter are recognized as true tumors.

The criterion of true tumors is regarded to be their tendency to permanency in contradistinction to inflammatory products, which tend to disappear. The cases collected and the views of reliable observers recorded in my monograph show this to be incorrect. It has been proven that tumors occasionally heal and disappear. On the other hand, it is well known that only acute inflammatory products tend to disappear, while many chronic ones never do disappear, and that the symptoms and cause of the latter are frequently less obvious than in the case of tumors.

The connective tissue which, in proliferating, constitutes the main bulk of elephantiasis and of the cirrhosis of organs and a good many other pathological tissues outside of tumors, never disappears.

Virchow properly considers elephantiasis Arabum and soft fibroma morphologically and etiologically identical, and in the same sense he does not admit any difference between the connective tissue of an advanced cirrhosis of organs and that of a diffused fibroma. In fact, we are only in the habit of calling a proliferation of connective tissue in the mamma an intercanalicular fibroma, because the connective tissue affects an external part, while a similar affection of the liver or kidney we term an inflammatory one—a cirrhosis. Why should we make such a distinction?

Gummata, tubercles (tyromata), lupus, the well-established products of inflammation, are unquestionably true tumors.

Lucke observed that sarcomata in young individuals occasionally grow as rapidly as acute abscesses, and have been frequently mistaken for the latter.

Tissues which are most liable to be the seat of inflammation are also the most *common seat* of tumors. Again, those tissues which do not participate in active inflammatory processes (ganglionic and striated muscular tissue) seldom or never give rise to tumors.

The extensive and careful statistics of Dr. D'Espine, of Geneva, show that the os uteri and the stomach are the most frequent seats of primary cancer, and they are also distinguished for their remarkable liability to catarrhs. Virchow has repeatedly pointed out in a catarrhally inflamed gastric mucous membrane the gradual transition to carcinoma, a fact observed also by Dr. J. H. Musser and myself.

The healing process in malignant tumors (wherever it occurs) is precisely the same as that of an ordinary granulating ulcer. Here and there, healing is accomplished by the additional formation of connective tissue,—*i.e.*, cicatrization.

But the most beautiful analogy between tumors and inflammatory products is demonstrable by the microscope, which led to the discovery of new and important facts.

2. *Clinical and statistical proofs.*

My own experience is limited, but in the cases of tumors in which I had the opportunity to get the history myself, or where I insisted upon an exhaustive anamnesis in cases of others, it was possible in nearly one-half of the cases to trace out a local inflammatory process preceding the tumors at some time or other. Sometimes it dated years back. Careful inquiries nearly always revealed some cause,—viz., an injury, long-standing irritation, mechanical or toxic, or an impaired or excessive use of the part, pressure, or a long-standing catarrh, or something of that nature.

It is also an established fact that those organs and regions of the body which, from their position and their function, are most exposed to injuries or irritation are the most usual seat of tumors. This is proven for the orifices of the digestive and genito-urinary tract, which are so much exposed to injuries and are also classical seats of especially malignant tumors.

Primary cancer of gall-bladder has been repeatedly traced to gall-stones; that of the urinary bladder to a similar cause.

For surface-cancers an inflammatory origin may safely be regarded as proven. I know of scores of epitheliomata which had been traced to little sores produced by injury. Nearly all those everlasting leg ulcers are epitheliomata.

It is just here that the influence of evolution and involution of tissue upon the variety of tumor does not hold good. Repeatedly have I seen epitheliomata of lower extremities in young persons directly produced by injuries, burns, etc. (Clinical service of Prof. Agnew.)

Who will deny the inflammatory origin of the very common epithelioma of lip? or that of tongue or penis?—the first nearly exclusively occurring in smokers, the second always being associated with injury of tongue by sharp teeth or otherwise, and the third with congenital or acquired phimosis.

Chronic inflammations of the skin, as occurring on workers in coal-tar and paraffin manufactories, etc., commonly lead to epithelioma. A similar origin has the chimney-sweeper's cancer.

It has been proven that long-continued catarrhs of stomach (particularly in drunkards) lead to cancer.

Through irritation and injury common warts and scars are produced; further repeated injuries very frequently convert them into malignant tumors (cancers, sarcomata).

Prof. Agnew removed from the back of a middle-aged person a sarcoma developed secondarily in a scar. Some time previously he had removed (from the same patient) from the same spot, or from above that spot, a lipoma.

Sarcomata are commonly due to direct injury; neuromata exclusively so.

Glioma and tyroma, as met with in the brain, are nearly always traceable to falls and blows.

Hundreds of cases of fibroma, lipoma, chondroma, and osteoma have been traced by distinct and clear histories to pressure and irritation, or directly to blows, fractures, cuts, and other injuries.

Winkel, who investigated exhaustively the etiology of fibromata and myomata of the uterus, came to the conclusion that these tumors are caused either by direct excitants, viz., coition, injury, abortion, rough removal of placenta, cellulitis, or, indirectly, through repeated lifting, shock, sudden hyperæmia, etc.

No reliable line of distinction can be drawn between the lymphomata and lymphadenitis.

Any one can convince himself of the above-mentioned facts by just looking carefully over the literature, and by taking careful histories of his own cases. Hundreds of tumor cases of positively traumatic origin are also recorded in the classical works on tumors of Virchow, Weber, Müller, and Broca.

Unfortunately, however, these facts are not generally known; the literature is not sufficiently studied, and the histories of tumor cases are not sufficiently carefully inquired into.

Inflammation is the only factor which has been traced to be the positive cause of tumors in a number of cases. This is proven by high authority and statistics. But as these authenticated cases of inflammatory origin are in moderate number, and as those with no cause (by reason of careless note-taking) are in enormous majority, the inference is drawn that inflammation has little or no significance in the pathogenesis of tumors.

I beg leave to argue as follows. In a certain number of cases it is positively known that inflammation preceded and was the cause of the new growth. In regard to the remaining cases of tumors we know nothing; no positive cause could be traced. Hence I think it logical, for the present, to consider inflammation as the cause of all true tumors. All other alleged causes are only speculations, and nothing reasonable can be brought forward against the inflammatory theory. Speculations are valueless, I think, in the presence of positive facts, even if these be few in number. In science any amount of negative results are always disregarded in the presence of even a few positive facts. *Until contrary proof be given, we are at present, by a mass of evidence, forced to the conclusion that tumors represent merely one of the terminations of inflammation.*

3. *Microscopic proofs.*—Here I will make the following abstract from my first paper:

The question now arises, in what way does inflammation produce a tumor, and why and when does a tumor develop after an injury? Why is not every injury followed by a tumor, if inflammation is the cause? Prof. Maas's* ingenious answer was that it depends upon the presence or absence of Cohnheim's supernumerary embryonic cells at the seat of the injury. If those misplaced or aberrant cells happen to be present in a part, a trauma will induce inflammation, followed by a tumor; if no extra cells are present, a simple in-

* Berliner Klin. Wochenschrift, No. 47, 1880.

flammation will follow, and nothing more. But this is only a hypothesis; it cannot be demonstrated. Embryonal (fœtal) cells could not continue to exist unchanged in the adult individual; nor do they need to be pre-existing in order to form a tumor. They can be and are always created by any inflammatory process.

I will try to answer the above question by facts which microscopic examination reveals, and which will show that the study of histogenesis must go hand in hand with that of the etiology and possibly might disclose the mysteries of the cause of tumor.

It is true that not always direct observation of active pathological processes can be made. In the case of tumors, only inferences of previous cell-activity can be drawn from the microscopic picture; but the pathological process can frequently be traced out under the microscope, from the various transitional stages of the elements of the new forming or formed tissue.

It is in accordance with the modern views to say that every tumor has its strict physiological prototype. Even for the cancer, only the peculiar atypical arrangement of the cells remained a criterion, while the cells themselves are supposed to be strictly identical with those found normally.

It appears to me, and the more I study the histology of tumors the more I become convinced, that any variety of cells composing a tumor are not identical with those found normally, but resemble those met with in chronic inflammatory products.

In tumors, the shape and the peculiar variations in size of the cells and nuclei, the character of the intracellular net-work and of the amœboid motion of certain cells, the intercellular substance, the occasional arrangement into nodes, the relation to reticulum and blood-vessels, and the peculiarity of the latter, are all precisely like what is found in chronic inflammatory products and not like in normal tissues.

There is a great difference between the tissue-elements of fibroma and those of normal connective tissue, for example.

I shall give briefly the details of my investigation of the structure of fibroma, which, when completed, will be published and illustrated elsewhere.

Concerning the structure of normal connective tissue, the following seems to be generally established and in good preparations quite demonstrable:

The ultimate connective-tissue fibrils (the fibrillar variety) are in varying number united together to form bundles; these again occasionally unite to form larger bundles; these bundles arrange themselves at different localities in various manner, *i.e.*, parallel as in tendons, or as a lattice-work in membranes, or decussate at different angles and in all possible directions in all other localities, leaving between small spaces, these spaces being dependent for their shape and size upon the arrangement of the bundles. They communicate with one another, and thus form a system of channels throughout the whole connective-tissue system of the body. These channels contain a small amount of fluid containing *mucin*, and they are the receptaculi of the sometimes enormous quantities of serum in œdema. These same spaces or channels may also get filled with air, producing emphysema in skin and other parts of the body.*

Von Recklinghausen has shown that the spaces in the connective tissue communicate with the lymphatics, and he calls the spaces juice-channels; they act as "vasa serosa" (Orth), conducting the serum from blood-vessels to the lymphatics, and "feeding" (Tyson) the tissues.

By the nitrate of silver method of Von Recklinghausen, which is now the common property of all the laboratories of the world, it can be easily demonstrated that each of the connective-tissue bundles spoken of is surrounded by a distinct membrane composed of large flat cells. These flat, so-called endothelial cells are very thin, nucleated, and are closely united at their periphery with one another, so as to form continuous membranes or sheaths, which envelop each or several fibrillar bundles and thus at the same time form a lining for the spaces between them. Without nitrate of silver the endothelial cells cannot be seen; all that is seen are the nuclei of the cells, round or oval in shape if viewed from above, or spindle-shaped if the whole cell is seen in profile. I will not enter into further details here; this suffices to make myself now intelligible concerning some points in the histology of connective-tissue tumors, particularly fibroma.

* The subcutaneous tissue of the whole body can be filled with air, so as to produce enormous emphysematous disfiguration, by forcing air through blow-tubes at a few points or possibly even from only one point of the body below the skin. I have seen children purposely prepared in this way for beggars' purposes.

I investigated by the nitrate of silver method three specimens of fibroma: 1st, a small, hard fibroma from the finger of a girl, æt. 20, developed from the tendon; 2d, one of the size of two fists from the broad ligament of a woman, æt. 35; and 3d, an intra-uterine fibroma of the size of one fist, from a woman, æt. 40.

I might say at the outset that in the preparation of the first and third specimens I failed altogether to discover any perfect endothelial sheaths surrounding the bundles of fibres, which were so beautifully seen in a preparation of tendon made for comparison simultaneously with the fibroma specimens. In specimen second only a few perfect endothelial sheaths were visible. The microscopic picture of one of the silver preparations (from specimen No. 1) was this. The fibrils were on the average much thicker than in normal connective tissue; some running straight, others rather wavy and not quite parallel with one another, frequently decussating. Only few perfect fasciculi or bundles of fibres were seen, but most of them had not a trace of endothelial ensheathment. Some had a partial endothelial sheath in some places, and here the bundles appeared constricted. In several places were seen irregular protoplasmic masses apparently in connection with the fasciculi and proved to be partially detached endothelial cells. Between the bundles were seen several groups of young indifferent cells, resembling white blood-corpuscles. Other cells were double the size of the latter, some spindle-shaped and with prominent nuclei. The latter were seen occasionally in a state of division or were already divided. They resembled remarkably the germinating endothelial cells from serous surfaces, as described by E. Klein of London, represented by him in his Atlas of Histology, Plate VI.

I interpret the microscopic picture as a whole thus. The endothelial cells composing the sheaths of bundles of connective tissue have become isolated, and hence the sheaths are destroyed. The boundaries being removed, the liberated connective-tissue elements grow with great vigor. The growth is perhaps promoted yet more by the presence of the serum of the juice-channels, with which the cellular and fibrillar elements now come in direct contact, the sheaths being destroyed. The cells and fibres here, like in elephantiasis, "feed" (as Prof. Tyson would say) upon that serum in which they are soaking. The endothelium is proliferating (germinating, *Klein*), and probably gives rise to those groups of indifferent cells which evidently form the main source of the new growth. Foerster* has pointed out that in the development of fibroma the fibres arrange themselves more or less concentrically around and develop from these islands of cells, thus giving rise to the lobulated appearance of this new growth. It is also very probable that emigrated white blood-corpuscles assist in forming those collections of cells.

What interests us at present, however, is the absence of the endothelial sheaths in the connective-tissue bundle in the fibroma, and that this feature fibroma has in common with all connective-tissue formations which owe their origin to inflammation, as will be shown directly.

I can affirm the absence of endothelial sheaths in the new-formed fibrillar connective-tissue as met with in cirrhosis of organs which invariably accompanies the proliferation of the alveolar connective tissue in such situations. It would be very desirable that other histologists would undertake research in this direction.

Cornil and Ranvier† describe the disappearance of the endothelial ensheathments in connective tissue which is the seat of inflammation. They describe the appearances as follows: "The fasciculi are smaller; less distinctly fibrillar; they do not appear to be enveloped by a special layer which limits them and which causes them to swell irregularly when acted upon by acetic acid." Cornil and Ranvier consider that the "large flat cells" are replaced by embryonic tissue.

The inflammatory process is, to my knowledge, the only factor which can disconnect or isolate endothelial or epithelial cells united together to form a certain lining or covering. Let us take, as an instance, the lung. The flat cells which form the lining of the air-vesicles are so closely united or grown together in the normal adult individual that no means at our command at present can isolate them. But in catarrhal pneumonia the inflammatory process demolishes that lining instantly, the cells which compose it "re-

* Atlas der mikroskopischen und pathologischen Anatomie, Leipzig, 1855.

† A Manual of Pathological Histology, translated by Shakespeare and Simes, Philadelphia, 1880.

turn to their embryonic state" (Stricker), they become completely isolated.

The abnormal increase in bulk of tissue in both the fibroma and the inflammatory connective-tissue products, appear to me to be due to the same cause:

1. The removal of the boundaries which keep the fibres intact, viz., the destruction of the endothelial ensheathments.

2. The proliferation of the endothelial cells of these destroyed sheaths and of the connective-tissue elements themselves, and probably with the aid of white blood-corpuscles.

If the endothelial sheaths of the connective-tissue bundles and other normal boundaries are re-established in the inflamed tissue, then it will return to its normal state, or in case of loss of substance will heal by permanent scar-tissue. The healing process was perfect.

On the other hand, the same tissue will give rise to a fibroma if this healing process was imperfect; *i.e.*, the endothelial ensheathments are not re-established, the connective-tissue elements remaining freed from any restriction proliferate on their own accord, grow above the physiological limit, and thus inflammation terminates in a tumor.

Hence, from histogenetic grounds, I would suggest that *fibromata should be classed as a product or rather as one of the terminations of inflammation.*

This is also in accord with clinical experience.

Now, is an inflammatory origin less evident in other tumors? Can there be shown any positive microscopic difference, for instance, between a mass of inflammatory granulation tissue and a sarcoma? It cannot. To my knowledge, distinguished histologists have repeatedly had sad experience in this.

If the discoveries of Classen and Woodward should prove correct, we would, to my mind, have another additional proof that cancer is only one of the terminations of inflammations. I will quote the following:

Woodward* says, "My own studies of thin sections lead me to the conclusion that the migration of white blood-corpuscles played a great rôle in the development of cancerous growths, and that at least in certain cases the cancer cylinders were formed by the transformation of these corpuscles; which first accumulated in the lymphatic capillaries and the passages leading to them."

Classen† is even still more positive, saying that he has proven "that the cells of cancer cylinders and all the elements of cancerous growths are no other than migrated white blood-corpuscles escaped from the blood-vessels."

Though in my own research I did not succeed as yet to confirm the observations of Woodward and Classen, they are possibly correct, and I utilize them as coming from such high authority. Besides, they correspond so remarkably to what I believe to have established for fibroma.

My view of the histogenesis of fibroma holds good also for primary glandular carcinoma.

The glandilemma or basement membrane in glands (wherever such exists), upon which the epithelial cells rest, may be destroyed in precisely the same manner as the endothelial sheaths of the fibrillar bundles. This is demonstrable in carcinoma beginning to develop in a gland, or in the transformation of an adenoma into cancer. Here, as in fibroma, only an inflammatory process can accomplish this destruction of the normal boundaries. These boundaries, if not re-established after an injury by perfect healing, there is nothing to prevent the epithelial cells from travelling into surrounding connective-tissue spaces and to thus form a cancer.

I have here reference to the destruction of the endothelial boundaries which forms the basement membrane of the epithelium alone. The endothelial ensheathment of the connective-tissue alveoli remains perfect in the cancers unless it becomes inflamed.

It is not the want of resistance of the surrounding tissue (as is generally held), but simply the getting loose of the normal cells from their place of attachment, which constitutes the formation of a malignant tumor.

It is the mobility of the cells, I think, that conditions the malignancy of a tumor. Any tumor, even the most benign one, would be eminently malignant if the cells composing it could get loose and travel through the widely open paths of the system of juice-channels.

* The Structure of Cancerous Tumors. Toner Lectures, Washington, 1873.

† Ueber Cancroid der Cornea, etc., Virchow's Archiv, vol. l., 1870.

In benign tumors the cells are more or less fixed, hence no metastasis. The endothelial basement membranes and ensheathments are, however, here also defective. The physiological boundaries which maintain the equilibrium and keep the cells in position and in harmony with one another are found absent in that tissue which gave rise to tumor-formation.

As it is not proven so far that any other pathological process besides inflammation is capable of destroying the endothelial ensheathments and basement membranes, I am driven to the conclusion that all true tumors are direct products of the inflammatory process, and that true tumors should be considered as one of the terminations of inflammation.

A NEW METHOD OF TREPHINING THE SKULL AND OTHER BONES.

BY JOHN B. ROBERTS, M.D.,

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A SHORT time ago I became cognizant of the method used by Prof. James E. Garretson for the removal of the coccyx. This he effects by uncovering the bone and grinding it away with the Bonwill surgical engine armed with a burr. A few days later, by his invitation, I saw him remove in a similar manner the right superior maxilla, which was the seat of an antral exostosis. The delicacy of manipulation, the absence of facial scarring, and the undoubted power of the engine, combined to give me a very high appreciation of its possibilities. Especially was this the case because my experience some three years ago with the so-called dental engine was very unsatisfactory in surgical operations on bone.

During Dr. Garretson's operation some one of the by-standers suggested to me that the engine might be used for trephining, and, as I had shortly before been teaching this operation to my class, I was struck with the idea. It has been heretofore suggested, I believe, that the engine might be employed to drive a trephine, and thus cut out a disk or button of bone.

My idea, however, was that, as the ordinary trephines are usually of too great diameter and cause larger openings than are required for the insertion of the elevator, it would be practicable to bore a small hole in the skull by using in the engine a burr cut or roughened on its flat extremity.

As no patient was at hand, I utilized a cadaver for the experimental demonstration, and fractured the skull by means of a hatchet. I found that the burr called by the dental instrument-makers a fissure-burr, and which has a cut face, answered admirably. I applied it to the sound bone at the edge of the depressed fracture, and found that I could quite readily make a circular cavity in the outer table. This was carefully deepened until the vitreous table was perforated. As there was no disk to remove, and as the burr, which I kept moistened with water, dropping from a cloth, threw out all bone-dust, the depth and character of the perforation were readily watched. When the skull was thus pierced by a round orifice about one-quarter of an inch in diameter, the elevator was inserted and the depressed fragments elevated and, where loose, removed. Sharp and irregular edges were equally well trimmed smooth or cut away by the burr.

When the rapidly-rotating burr is placed in contact with soft tissues, as one's finger, it can be pressed upon with considerable firmness without abrading the surface, while osseous tissue is quickly ground away. Hence it seems as if the meninges of the brain might be touched by the burr without injury being inflicted at the time the vitreous table is perforated. In fact, I am inclined to believe that the dura mater would be pushed in front of the burr and remain practically uninjured. This can only be tested in living animals or human beings, because in the cadaver the brain does not entirely fill the cranial cavity, though the dura mater may remain attached to the inner table. The depressed fracture, moreover, usually pushes the dura mater downward, which would thus be likely to be torn off from the sound bone nearest the depression.

The ease and success with which the long bones, containing abscess cavities, could be perforated by this method are unquestioned. My experience in once breaking the handle of the ordinary trephine, while endeavoring with difficulty to bore into an abscess in the head of the tibia, makes me hail the improvement with satisfaction.

The method of trephining the skull with the surgical engine of Bonwill, which I believe to be the only one sufficiently powerful, would then be as follows. Pick out

a burr one-fourth or three-eighths of an inch in diameter, well tempered, and having a flat face deeply cut; then, fixing it in the mandrel, close up to the hand-piece, have the engine-crank turned with great rapidity. When the skin and periosteum have been dissected up, apply the burr to the sound bone nearest the *most* depressed portion of the fracture, and at first tilt the burr a little on the edge until a shallow groove has been made on one side of the proposed perforation. This prevents the burr slipping from the smooth convex cranium. Keep the burr constantly moistened by means of a wet sponge held over it and occasionally squeezed. When the perforation has been made, use the elevator as in ordinary trephining. If there is difficulty in elevating or removing the fragments, cut away with the burr the edges which cause locking. Hey's saw or bone-cutting forceps will not be required.

The ordinary burr furnished by makers of the engine is sufficient, but the face could with advantage, I think, be cut deeper. Instead of the ordinary burr the central portion of the face might be bored out, leaving then a burr that would remove a disk like the ordinary trephine does, and which might also be made conical. These changes, however, would be of doubtful advantage, though I shall probably experiment with this form of burr.

The use of the surgical engine for perforating the cranium is, as far as I know, novel, but it is very possible that others may have experimented on the cadaver or living subject and found similar results.

NOTES OF HOSPITAL PRACTICE.

UNIVERSITY HOSPITAL.

CLINICAL SERVICE OF PROFESSOR WILLIAM GOODELL, M.D., PROFESSOR OF CLINICAL GYNÆCOLOGY.

Reported by GUY HINSDALE, M.D.

A CASE OF ABDOMINAL TUMOR.

GENTLEMEN, — Our next patient comes here saying that she has been troubled by pain in both sides, and that she has a "swelling in her stomach." She thinks it is due to the change of life. She is 45 years old, and is now losing her monthlies. This function is now in what may be called the dodging period, when the menstrual flow comes and goes at irregular intervals, and when various nervous symptoms are liable to be developed. There is at each menstrual period a turgidity of all the reproductive organs, giving rise in some cases to chronic ovaritis and ovaralgia, ovarian epilepsy or insanity, neurasthenia, and, in short, all those phenomena or lesions which can be traced to ovarian or spinal congestion, and which are embraced under the heading of pernicious menstruation. They present true nervous symptoms which often refuse to yield to every kind of treatment, proving wholly unmanageable until the climacteric has become established.

As I examine this case, I find the cervix very high up: I can just reach it. There is also a laceration upward and to the right as far as the vaginal junction. The cervix is very soft. What is the meaning of a soft cervix? What does that imply? It means that you mustn't pass the sound up. There is a possibility of pregnancy. You see, I am thinking aloud. The patient was married at forty, is now forty-five years old, has been pregnant, but never went to full term. Her last miscarriage occurred two years ago. She complains of something like heart-burn. The cervix is high, as though there were something in the womb. Here is a growth, an enlargement which reaches one or two fingers above the navel, which is too high for a pregnancy of five months, but which might occur shortly after the end of the sixth month. I am feeling to see if there is any hard body, but she is so very fat that it is not easy to recognize a definite outline. There is an elasticity about it which leads me to think that it contains fluid; but this I cannot positively determine. As my hands press the surface of the tumor, I fancy that I feel alternations of softness and hardness, which is one of the characteristics of the gravid womb. I will try to get the fœtal heart-sounds; but I can hear nothing. Her nipples show a little moisture, and the follicles are enlarged; that strengthens the supposition that she is pregnant, but is not absolute proof. As I manipulate the breast this moisture increases and becomes actually milky, and the nipples become erectile. If you look carefully you will observe that the linea alba is darker than the surrounding skin. Dr. Cormack has drawn attention to this dark line, which occurs during pregnancy in the median line of the abdomen. It is constant in the latter

part of gestation, but, being the result of distention, it is associated with other abdominal tumors, and, indeed, we often see a darker line.

Another point is of value in the diagnosis: the change of life does not come in an abrupt way. Menstruation, instead of stopping at once, becomes irregular; it dodges. The woman at the approach of this period may not menstruate for six weeks, after which it comes on as before; then she will skip an entire month. Disorders and perturbations of one kind or another characterize this change, and often continue for two or three years or more.

But this tumor extends above the navel, and it is elastic: that strengthens the idea that she is pregnant. Then we have the soft, very soft cervix. When the cervix is as hard as the tip of your nose, you may safely conclude that the uterus is empty; but when it is as soft as your lip, it should always make you cautious. She does not have morning sickness, but complains of heart-burn. She has to get up at night to pass her water; she says she did not have this trouble before Christmas. Women, when they are pregnant, will have to get up once or twice in the night to pass their water, and this sometimes has to be done more frequently towards the end of gestation. I will pass one finger in front of the cervix to see if I can feel any hard body floating in a fluid and get *ballottement*. I cannot get the characteristic feeling; and yet the tumor is a suspicious one. This woman never had a child at full term, and what at first I took to be a laceration I should say is only the softened lips of the cervix. This woman's color is good, and we can exclude malignant disease: I am, therefore, disposed to consider her pregnant. It will be safer to do so, and to await developments.

The diagnosis of pregnancy is not always an easy matter. In many cases it is difficult to avoid being deceived. Designing women will come to your office hoping to mislead you because you are young. They will say or do everything they can to lead you to undertake an examination or make some application that will induce abortion. Remember also that pregnancy in the unmarried is an accident that will happen in all classes of society. As a general thing, the family physician is misled, and he is misled because he is intimately acquainted with all the members of the family and cannot believe that any of them could be led astray any sooner than one of his own family.

To illustrate this point, let me tell you a story. A good many years ago, I won't say how many, I was called in by an elderly physician to examine a tumor in a young lady. It was steadily growing, and I was asked to decide what it was, and whether it could be removed by an operation. The house was so handsome, the surroundings so fine, and the mother and daughter so elegantly attired, that I could not for a moment entertain the idea that the girl was pregnant. So I was completely thrown off the scent, and when we went down to consult over the case we decided that it was some abdominal tumor of doubtful nature, and, if I remember correctly, we did what many physicians often do when in doubt,—but which you will not, of course, do,—we prescribed minute doses of calomel. While on my way home, when I was no longer under the glamour of laces, velvet carpets, costly furniture, and a brownstone front, I began to rehearse the symptoms in cold blood. "What means," I said to myself, "a vagina so distended that I introduced two fingers into it? What means the absence of a hymen? Why are her lips red and her general health so good?" I felt so certain now that she was pregnant that I called myself all manner of names for my stupidity. That evening I hastened to the office of the physician to make a clean breast of my suspicions, and, after reciting all the above symptoms, I said, deliberately, "Doctor, that girl is pregnant!" He instantly jumped up from his chair as if he had been insulted, and said, "Impossible! impossible! why, I delivered her mother of that girl!" What he implied by these words was, "I brought that girl into the world: I have known her too well to believe it possible for her to have been led astray." He was too much moved and too deeply offended to listen to reason: so I was not slow to take my departure.

The upshot of the affair was that the girl was shortly afterwards sent away from her home to be secretly delivered, that my friend the doctor, up to the day of his death, never called me again in consultation, and that the various members of the family studiously avoided recognizing me in the street. One does not burn one's fingers twice in the same way, and since

that time I have never been caught in a like scrape.

Knowing these facts as well as I do, let me end this the last lecture of the spring course with this note of warning: never pass the sound till you are sure that the womb is empty.

TRANSLATIONS.

HÆMOGLOBINURIA FROM NAPHTHOL.—Naphthol, which has recently been suggested as a remedy in certain diseases of the skin, has been found by A. Neisser (*Cbl. f. Med.*, No. 30, 1881) to have a toxic effect when introduced into the blood in considerable quantity. Neisser has shown by experiments upon dogs and rabbits that large doses of this substance give rise to hæmoglobinuria. Rabbits weighing one thousand grammes died after the subcutaneous injection of one gramme of naphthol in concentrated solution in warm oil. Dogs of four thousand five hundred grammes died after doses of 1.5 gramme had been administered. Death occurred in two and one-half to twelve hours, following salivation and restlessness in the case of the dogs and convulsions in the case of the rabbits. The fact was observed here, as in the case of pyrogallic acid, that dogs could not bear as large doses as rabbits. This was in curious contrast to the fact that the human organism is more susceptible to pyrogallic acid. Neisser therefore warns against the excessive and too extensive use of this drug. Careful examination of the urine should be made from time to time during the administration of a course of naphthol.

CHRYSAROBIN.—In the last number of the *Times* we gave an abstract of Israel's investigations on the nature and local influence of chrysarobin. The latter part of his article on this subject is devoted to a consideration of the influence of this drug upon the economy generally. It is known that such substances as rhubarb and senna, which contain chrysophanic acid, give rise, when taken, to a yellow discoloration of the urine, which changes to cherry red on the addition of alkali or when the urine goes on to decomposition. An examination of the urine, therefore, will show whether or not the remedy has been absorbed in any given case, and what its transformations in the body have been. Thompson has shown that when chrysarobin is given internally in pill form or in water, followed by alkalies to promote absorption, it gives rise to vomiting and diarrhœa. Other observers failed to find any effect, either when the remedy was given internally or when rubbed into the skin. Israel, however, has instituted independent researches in this direction. He gave pure chrysarobin to animals and found chrysophanic acid in urine and fæces. Blood was also observed in the urine, due, Israel thinks, to the irritative effect of a certain amount of unaltered chrysarobin which was excreted with the chrysophanic acid. He thinks that in this respect chrysarobin is irritating to the mucous membranes, just as cantharides is. In order to investigate the action of the remedy when applied externally, Israel rubbed into the integument covering the belly of dogs, previously shaven, an ointment containing one part of chrysarobin to fifteen of fat, covering the part then with an impermeable dressing. Investigation of the urine on the third day showed chrysophanic acid, and consequently proved that chrysarobin can be absorbed through the skin.

In conclusion, Israel says, upon the ground of frequently-observed albuminuria following the inunction of chrysarobin, that this circumstance must be borne in mind when administering the remedy. Attention does not appear to have been drawn thus far to this occurrence; but should it be found that albuminuria is at all common after the application of chrysarobin, the occurrence must be guarded against.—*Virchow's Archiv*, Bd. lxxxv. p. 124.

CRITICAL EXAMINATION OF SOME WHOOPING-COUGH REMEDIES.—Prof. Otto Heubner (*Deutsche Med. Wochens.*, 1881, p. 541; from *Jour. f. Kinderkr.*) has made an investigation of the five most highly recommended remedies in whooping-cough,—namely, bromide of potassium, quinine, hydrate of chloral, salicylic acid, and belladonna,—with a view to ascertaining their exact therapeutic value. Heubner prefaces his paper with the remark that it is much more timely to make a thorough examination of the effect and value of such remedies as we now possess than to go hunting after new specifics. He selected whooping-cough because it is easy to diagnosticate with certainty. In addition to this, the cases to be studied were taken from patients in the same neighborhood and social posi-

tion, from uncomplicated cases, and from all the cases as they presented themselves at his clinic. The remedies used, except belladonna, were such as are apt to be obtained of uniform character, thus eliminating one element of uncertainty. The action of the remedies was studied in three directions,—(*a*) in relation to the intensity of the individual attacks, (*b*) in relation to the frequency of the attacks within a given period of time, and (*c*) in relation to the entire duration of the disease. Six weeks was taken as the average duration of the disease; and if the medicine failed to shorten this the result was counted negative, while if the duration of the disease was shortened it was counted positive. Forty-four uncomplicated cases of whooping-cough were studied.

The drugs were given as follows. Bromide of potassium in doses of 0.5 to 3.0 grammes in watery solution per diem. Quinine was given in solution or in powder in the dose of 0.3 gramme per diem. Chloral hydrate was given in two cases in broken doses, in the other cases in enema, in the dose of 0.3 to 1 gramme per diem. Salicylic acid was given in one case in the form of salicylate of sodium, inwardly; in the other cases it was inhaled as spray in a one-third- to one-half-per-cent. solution, 0.1 to 0.15 gramme of salicylic acid being inhaled at each sitting. Belladonna was usually given as the powdered extract in doses of 0.015 to 0.06 gramme per diem.

The results of Heubner's investigations are given in the following table.

Influence on the Attacks.			Influence in shortening the Disease.		
	Positive.	Negative.		Positive.	Negative.
Salicyl. inhal	10	7	Belladonna	3	5
Chloral	6	4	Quinine	3	8
Belladonna	4	4	Chloral	2	8
Quinine	5	6	Salicylic	2	15
Bromid. potas	9	14	Bromid. potas	0	23

From the above table of cases it appears that salicylic acid is about eight times as likely to be useful in diminishing the frequency and severity of the attacks as is bromide of potassium. Salicylic-acid inhalations are therefore the best means of shortening and diminishing the attacks, while belladonna and quinine have the best effect in abbreviating the duration of the disease. It must be remembered, however, that the best of these remedies fail to diminish the number of attacks by one-half, and any new remedy which may be brought forward should be carefully put to the proof by these methods.

Hot Water in the Treatment of Hemorrhoids.—Landowski (*Cbl. f. Chir.*, 1881, No. 38; from *Jour. de Thérap.*) suggests hot sitz-baths in bleeding piles, together with enemata of hot water. These not only check the bleeding, but diminish the size of the turgescent tumors to a marked degree. In ordinary hemorrhoids three sitz baths per diem may be employed. In bleeding piles the baths should be more frequent, and the enemata should be given as hot as the patient can bear (usually about 104°).

Effervescing Draught of Bromide of Potassium in Vomiting.—Dr. Chéron (*La France Médicale*, vol. ii., 1881, p. 464), having tried various remedies in that form of vomiting which accompanies ovaro-uterine complaints in women, finally settled upon the following:

No. 1. ℞ Potass. bicarb., ʒss;
Aquæ, fʒij;
Potassii bromidi, ʒss. M.

No. 2. ℞ Acidi citrici, ʒj;
Aquæ, fʒiv;
Syrupi simplicis, fʒx. M.

Pour a teaspoonful of No. 1 into a glass, and add a tablespoonful of No. 2; stir them together, and drink while effervescing. The dose may be repeated every hour or every half-hour, but the amounts given above in Nos. 1 and 2 represent the total quantity to be taken in twenty-four hours.

Liniment in Prolapse of the Uterus.—In prolapse of the uterus, M. Chéron, of the St. Lazare Hospital, besides the application of a pessary to keep the organs in position, prescribes the following liniment in order to ease the neuralgic pains from which many patients suffer:—chloroform, three drachms; ether, four drachms; camphorated spirits, three ounces. These frictions on the lumbo-sacral region are attended with the best effects. Also to restore the tone to the relaxed ligaments he gives—bromide of potassium, a drachm and a half; tincture of iodine, fifteen drops; tincture of aconite, twenty-four drops; syrup of tolu, ten ounces. A tablespoonful before each repast.—*Medical Press and Circular.*

PHILADELPHIA
MEDICAL TIMES.

PHILADELPHIA, DECEMBER 31, 1881.

EDITORIAL.

THOROUGHNESS.

ONE of the needs of the medical student, not less of the practitioner, is *thoroughness* in minor cases. The medical student indifferently turns his back upon the clinical treatment of a felon or an abscess. A headache, constipation, lassitude, as independent complaints, meet with equal indifference from too many physicians.

The student will neglect everything else and go any distance to witness an amputation at the hip-joint; but if the surgeon present a broken finger, or if the physician bring forward a dyspeptic patient, the student, calmly superior to such frivolities, directs his mind to the consideration of a cigarette or idle gossip.

A patient complains to his physician of a headache or some equally slight and common ailment. "Ah!" says the doctor, and scribbles off a prescription, with the remark, "Take this, and by to-morrow you will be all right." Perhaps. The chances, however, are that on the morrow the ailment remains unchanged. Another visit follows, and on this occasion the doctor, being obliged to think, asks a question or two, and changes the prescription, with, it may be, a like result. At his third visit the client, whose patience and pocket alike begin to suffer, suggests (and *could* there be anything more embarrassing?) that the doctor has not discovered the *cause* of the trouble. This puts the physician on his mental muscle, if he have any; and now begins the proper examination of the case. A skilful cross-questioning reveals the error which has caused the patient's ailment, and this time he receives not only the needful and proper advice, but, if he now require a prescription, the remedy which will help him.

Dare any one say, unless this physician abandon his routinism and his automatism in minor cases, that he will not in time win a reputation of indifference to small matters? He may be strong in the treatment of some dread malady to which he has given profound thought and in which he is deeply interested. This will give him the name of being the man for diphtheria or typhoid or pneumonia, but one patient will be sure to say to another, "Don't go to Dr. X. Y. Z. with little troubles; he is not interested in anything less than a case of life or death." Nevertheless, Dr. X. Y. Z. ought to know that just as the "mosquito troubles" of life are those which mostly annoy the mind, so do petty aches and small distresses form the bulk of the complaints from which the patients of the general practitioner ask relief. The doctor who is the most successful in the treatment of minor ailments will, it is true, be apt to be equally reliable in cases of greater import; but the truth is that his management of light cases is what wins his patients' confidence.

There can be no greater relief to the mind of a man who is the victim of some slight (to him grievous) but obstinate affection, and who has vainly sought relief at the hands of a dozen doctors who follow the automaton plan, than the keen, wide-awake interest and searching questions of the thirteenth doctor. At last he has found a physician who is interested in his case, and he at once feels that he will be helped. As he contrasts this thorough management with the half-hearted, half-asleep method of the foregoing doctors, he sees *one* explanation of their failure to give him relief. The new physician will be sure to keep him, and through him will win other patients.

The student who neglects the common and oft-recurring cases in which diagnosis seems to him a simple thing and, more-

over, the feature of most importance, will, when he has to deal with such cases unaided, find that he is weak just where he should be strong. He will discover that indifference to opportunity now gone by endangers his chances to make a reputation, whereas the capital operations to which he gave some attention do not come to him, or, if they do, it is in such paucity of number that he trembles for his financial future. The best student is he who makes himself familiar with minor matters, with small details in diagnosis and treatment. The larger features of cases soon become easily evident. It is the trifles which he needs to know; it is, too, the simple complaints which he should master. "Trifles make perfection," not only in artists, but in physicians as well. Neglect or ignorance of them has brought many a doctor to sore grief.

The medical man who wishes to be truly successful will, beginning in his student days, make the most petty detail a large factor in the treatment even of the simplest case. Every patient has a right to the best his physician can give him; but, unfortunately, a lack of *thoroughness* is one of the largely-prevailing causes of reproach to the profession. This should not be. It need not be.

M. TOUSSAINT, of France, claims that he has found the micro-organism which produces tuberculosis, and that he has not only succeeded in isolating the plant, but in producing with it, after culture, the disease. Until his experiments are published in full, destructive criticism may well be suppressed, but incredulity may very properly call attention to the established fact that it is possible to produce tuberculosis in some of the lower animals by placing clean sand, fragments of wood, and other inert, inorganic, or non-living materials in the muscle.

LEADING ARTICLES.

COLOR-BLINDNESS.

THAT this anomaly has long existed we cannot doubt; but at first sight it does seem curious that no actual determination of its presence was methodically made until within the past few years; but if we consider the unstable and contradictory theories of colors, optics, and physiology which have been promulgated by such men as Wünsch, Young, Goethe, Maxwell, Helmholtz, and Hering, the petty jealousies, the endeavors and claims for priority, and the search for hypotheses, all giving rise to half-fledged theory,—if we consider the fact that, at the time these were being solved, there arose an instrument which, sphinx-like, propounded new riddles* and revealed a new world,—we can easily see why this so-called medical curiosity must of necessity have remained unstudied and forgotten.

The earliest mention of a possible case of color-blindness was that by Dr. Tuberville, referred to in a letter to the Royal Society, London, August 4, 1684, in which he says, "A maid two-and-twenty years old, came to me from Banbury, who could see very well, but no color besides black and white," etc.†

The first authentic account is that by a Mr. Huddart, in 1777 (vol. lxvii., Philosoph. Trans.), who speaks of an intelligent shoemaker who, at about the age of four years, had his attention first called to his infirmity by his being unable to recognize a peculiarity which others attached to a certain stocking in calling it red. Later, he observed that he was unable to differentiate red cherries from green leaves, except by form. It was afterwards found by scientific investigation that he was almost wholly void of color-sense.

Many instances follow, especially the well-known case of John Dalton, of which Dr. Oliver Wendell Holmes might aptly say, "it has been clattering down the highway of fame like a dog with a tin kettle hanging to its tail," "Daltonism:" thanks to Dr. Jeffries for helping to unfasten the impediment, and making use of a better but still imperfect term,—color-blindness.

Although many theories of color-sense

* Dr. A. Geissler, Schmidt's Jahrb., 1881, Bd. 191, Hft. 1.
† Quoted in "Color-Blindness," etc. By B. Joy Jeffries, 1879, pp. 308.

have been advanced since the remotest antiquity,* yet not a single one has been or can be fully accepted.

Briefly stating a few of the most important—such as that of Wundt,† of a chemical chromatic and achromatic excitation; that of Lederer,‡ of differences of actions in the excitation of the rods and cones; the curious fancy of Professor Delbœuf,§ of the retina being a vibrating membrane set in motion corresponding to the velocity of impinging wave-lengths; the wonderfully ingenious theory‖ of Krenchel's movable color-molecules situated in the cerebral color-centres, which ignores every other part of the visual apparatus as in any way capable of differentiation—we pass to those of Young-Helmholtz, and Hering. The first, the so-called three-fibre theory, was brought forward by Thomas Young,¶ about the year 1800; it consisted in the supposition that each sensitive nerve-filament may consist of three portions, one for each principal color, and so acted on, either singly or in combination, as to produce the pure colors with their many tones and tints. Although Maxwell** amplified this already modified theory, it had remained for Helmholtz, in 1852, to alter the number of spectrum colors,†† but afterwards to suppose that every color seen is produced by the simultaneous excitation of all the three nerve-filaments in different degrees,—this being illustrated by means of a chart.

Both Rose‡‡ and Fick§§ bring serious objections against this theory.

The second is that of Hering.‖‖ First, assuming that black is a sensation and not the negation of light, he produces three paired primary sensations (blue and yellow, green and red, black and white), all being the results of an action of a "visual substance" situated in the retina; each paired sensation consisting in the loss or gain of a certain percentage of its part of the visual substance,—a dissimilative and an assimilative action.

If we were to accept and endeavor to use any of these theories in the explanation of color-blindness, we would find many discrepancies; but, as no better hypotheses of color-vision have been offered than those last named, we are obliged to conciliate, as much as possible, the theory with the abnormal condition, until such time as microscopic study of histological and pathological conditions of the cerebral centres and visual apparatus, together with proper interpretation of subjective differences of color, shall throw new light on the subject and thus substitute fact for supposition.

We are now brought to the question, What does a color-blind see? If we should ask a red-blind person to look at a piece of red worsted or a stick of red sealing-wax, what will he tell us? He says he notices a peculiar difference in its *shade* which characterizes it from all other sensations, and his description of it will depend entirely upon the extent and position of his visible color-spectrum. It must be remembered that he has no language for red as we see it: although he may learn by association to call a red object by its proper color-name, yet he is unable to recognize that peculiarity which to a normal eye so strongly contrasts it with the other colors; the same being true of all the other forms of color-blindness. Geissler says,¶¶ for the red-blind the landscape has the character of autumn. A sunset or the beauty of our red flowers, with their various tones of blue and violet, he is perfectly indifferent to; but his fine sense for gray-white-blue shadows gives to glacial views a greater charm than to the normal eye.

Why should he be pitied? He has never known the color. It is to him what an unknown sense might be to us: we are contented, not knowing better, and he the same. Again, his bichromatic world, with its innumerable shades, may be a greater source of delight than our recognition of another color sensation.

Turning to the practical side of the question, we find that George Wilson, of Scotland, was the first to recognize the importance of its discovery in those who are placed in such positions as to imperil human life by the improper interpretation

* Compare Helmholtz, "Handb. d. Physiol. Optik," 1867, pp. 267–272, and Goethe, "Zur Farbenlehre," 1810.

† Wundt, "Rudiments of Physiological Psychology," 1880, Bd. 1, p. 454.

‡ Kosmos, 1879, Bd. 4, pp. 438–457.

§ Schmidt's Jahrb., 1881, Nr. 7, p. 84.

‖ Arch. f. Ophth., 1880, xxvi. 1, pp. 91–102. (Ueber die Hypothesen von Grundfarben.)

¶ Philosoph. Trans. Royal Soc., 1802, p. 12. (Bakerian Lecture.—On the Theory of Light and Colours.)

** Philosoph. Trans. Royal Soc., 1860, vol. x. pp. 404 and 484.

†† See Helmholtz, Handb. d. Physiol. Optik, 1867.

‡‡ Arch. f. Ophth., 1860, vii. 2, pp. 73–108 (Ueber stehende Farbentäuschungen).

§§ Handb. d. Physiol., von Dr. L. Hermann, iii. 1, p. 199.

‖‖ Sitz.-Ber. d. K. Akad. d. Wiss., 1874 (quoted in Schmidt's Jahrb., 1881, Bd. 191, Hft. 1, p. 82).

¶¶ Schmidt's Jahrb., 1881, September, p. 101.

of colored signals.* By his examinations he induced but one railway company to test all their porters for color-blindness.

Although Dr. Favre, of Lyons, periodically examined all its railroad employés from the year 1855 to 1877, also calling the attention of the "Conseil de Santé des Armées" and the medical societies of Lyons and Marseilles to its significance and importance of study, it remained† for an accident on a Swedish railroad, which was merely supposed, without direct substantiation, to have been caused by the misinterpretation of the usual signals, to become the impetus for a new onset against the color-blind.

On the night of November 15, 1875, a collision between two express-trains occurred at Lagerlunda, in Ostrogothia, in which one passenger and eight employés were killed, and one passenger and one employé wounded. Upon inquiry, Holmgren says he felt justified in the assertion that the accident was mainly caused by color-blindness. Geissler says‡ that Holmgren does not give any ground for this assertion, and also that the Swedish Report of Railways, which appeared one year later, did not assign any cause to the accident.§ Anyway, this occurrence was felt throughout Europe, and strenuous efforts were everywhere made to avoid a second catastrophe. Statistics were compiled, theories sought, and methods of determination augmented, until the German- and French-speaking medical world overflowed with material. State railroad commissions and individual corporations were asked to aid; the government navies and mercantile marine were induced to give support; whilst valuable tables were gotten from the schools and well-trained military. All these helped to forward the movement and give it a substantial foundation, thus placing the subject upon a scientific basis.

The question now arose, that having a certain percentage of color-blind among those employed, which of the two courses to pursue,—change the entire method of signalling, or use none but the normal-eyed.

Upon railroads, the exhibition of white light indicates "clear track;" green, "slow up;" and red, "track closed." Thus we see that the great danger is in mistaking either red or green for white. Suppose a heavy fog, snow, or rain-storm should partially obscure the signals, the red-green blind, not seeing these colors as others perceive them (he depending upon intensity), would possibly think his track clear and "go ahead" at full speed.

In the marine service the danger is increased. All vessels carry a green light on the starboard side, and a red light on the port, so boxed as to be seen forward and amidships; accompanied by a low white forelight, and sometimes a high white aft-light. Hence by comparison we can easily distinguish a vessel's course. If the conditions of a heavy fog, snow, or rain-storm were to exist, we can see how a color-blind, judging these important colors by their intensities alone during the best of weather, is here placed in a position almost amounting to the entire absence of signal.

Can these signals be so changed or modified as to throw out the possibility of a mistake by a color-blind? Both observation and experiment have brought the universal dictum, There is no better method. Thus admitting the maintenance of this form of signalling, we must of necessity obtain eyes capable of accurately perceiving and differentiating the colors used. Again, if it be true that, as Jeffries asserts, "one male in twenty-five is color-blind in a greater or less degree," and "of this defect they may even themselves be wholly unconscious," there should be such statutes established as would eliminate all eyes with faulty color-perception from places requiring perfect color-sense.

There are many occupations open to the color-blind, and even a few where his infirmity would serve as an actual advantage,—in photography and lithography, wood-cut, steel- and copper-plate engraving, and printing,—in all of these, where there is the want of an organ able to accurately differentiate between the many shades. Why could not a color-blind artist limit his productions to winter landscapes, lofty mountain-scenery, studies in grays, thus obtaining wonderful and almost life-like

* The Month. Jour. of Med. Sci., 1853, vol. xvii. pp. 773 and 194, and 1854, vol. xviii. pp. 30, 309, and 411.

† Fronmüller, in 1863 (Schmidt's Jahrb, cxxiii. p. 213), hinted at the importance of normal color-sense for the recognition of signals. Besides several other French and German notices.

‡ Schmidt's Jahrb., 1881, Bd. 191, nr. 7, p. 106.

§ It may be interesting to note that Dr. Gintl, Central Inspector of the Lemberg-Czernowitz-Jassy Road at Vienna, a man of vast experience and knowledge in railroad statistics, is made to say that only a single accident traceable to faulty color-sense was known to him, this being on a Finland railroad between Helsingfors and Tawastehus in July, 1876, caused by a color-blind switch-tender who showed a green instead of a red light.

fac-similes of nature? Why need he despair and complain, when such a field of study and pleasure is thrown open to him? —a character of work in which, by necessity, he must surpass all others who do not possess this now properly termed, to him, *an advantage.* CHARLES A. OLIVER.

PROCEEDINGS OF SOCIETIES.

PATHOLOGICAL SOCIETY OF PHILADELPHIA.

THURSDAY EVENING, OCTOBER 27, 1881.

The PRESIDENT, DR. S. W. GROSS, in the chair.

DISCUSSION OF DR. FORMAD'S PAPER ON "THE ETIOLOGY OF TUMORS."

DR. S. W. GROSS said that the main propositions propounded by Dr. Formad were, first, that all tumors are the products of the inflammatory process, and, secondly, that in the development of tumors there is a destruction of normal boundaries. Gumma and tubercle are regarded by the author as tumors; but Dr. Gross thought that the term should not be applied to the temporary products of specific inflammations, but that it should be restricted to permanent additions to the normal tissues. Dermoid cysts ought certainly to be included, as should also angeioma and lymphangeioma. The former naturally comes under the classification of cystic growths, while the latter—in regard to the causation of which Dr. Formad confesses that he has strained a point—are not merely congenital enlargements of pre-existing vessels, but are composed partly of newly-formed vessels, and should, therefore, be retained among the neoplasms. While it is true that in carcinoma of the breast the membrana limitans, or glandilemma, of the acini and ducts is destroyed, it is equally true that it remains intact in adenoma of that organ: so that, in the formation of tumors, normal boundaries are by no means always destroyed. Dr. Gross was convinced that inflammation, or a process nearly related to it, plays an important part in the etiology of many tumors, but he thought that Dr. Formad was too exclusive in his theory. He, moreover, believed that Dr. Formad was too sweeping in his assertion that inoculation with the juice of neoplasms was incapable of begetting similar growths. Dr. Formad, indeed, quotes several experiments which disprove his own positive assertions; and Dr. Gross related the following cases, which he thought supported the doctrine of the inoculability of tumor-juices.

The first case shows that sarcoma may be transmitted to man from an animal, and the second and third demonstrate infection in the same individual other than by metastasis.

Case I.—An ox was affected with a subcutaneous tumor behind the scapula, which proved, on removal, to be a medullary sarcoma. A few days before the operation the owner made an incision into the swelling on account of pseudo-fluctuation, and there was a sanious discharge for some days. The wife, æt. 23 years, was in the habit of cleansing the part, and had at the time a small wound on the outer side of the fourth finger of the right hand. In a few days a small warty excrescence was noticed on the finger, which soon became the seat of burning pain, and attained a diameter of fifteen millimetres in a month. It was covered by a whitish-gray pellicle, and Dr. Kuhn, of Niederbronn, found it to be a medullary sarcoma. (*Magazin für die Gesammte Thierheilkunde*, 1862, p. 328.)

Cases II. and III.—Dr. Reinecke, of Hamburg, has recorded two examples of the inoculation of the canal formed in tapping the abdomen in carcinomatous peritonitis. In both the primary disease was cancer of the ovary, with secondary affection of the mesenteric glands and the peritoneum, resulting in ascites, for which paracentesis was performed, five times in the first case and twice in the second. In both instances cancerous nodules appeared in the track left by the trocar, which, on post-mortem inspection, were not found to be continuous with the carcinomatous peritoneum, but separated from it by a layer of sound tissue. (*Virchow's Archiv*, Bd. l.)

Dr. TYSON thought that whatever else might be disputable as to Dr. Formad's view of the etiology of tumors, he was correct in saying that there were more facts in support of the inflammatory view than could be adduced by the advocates of other theories. This much he was willing to concede, but still thought the proposition not proven. The dyscrasia theory has been practically disproved by Virchow. The spontaneous theory has some points in its favor, and it cannot be *disproved*, although, on the other hand, it has fewer facts in its favor than has the inflammatory view. Cohnheim's theory has nothing in its favor beyond the occurrence of rhabdo-myomata, dermoid cysts, angeiomata, and lymphangeiomata, etc., which are allied congenital growths. The inflammatory view has two sets of facts in its support,—viz., the occasional operation of causes which are identical with those which produce inflammation, and the histological resemblance presented by certain tumors, as fibromata, to the products of inflammation as seen in cicatrices, and, if Dr. Formad's last observation is correct, the further histological similarity as to the absence of the limitary endothelial membrane surrounding the connective-tissue bundles. Dr. Tyson agreed with Woodward and others in thinking that the time had not yet come for a satisfactory determination of the etiology of tumors. Certain facts adduced by Dr. Formad have not the weight that he sup-

poses,—viz., the want of permanence of tumors and the persistence of inflammatory products. The instances of both related are but exceptions to the rule. On the other hand, however close may be the resemblance of the histological elements of some tumors to those of inflammation, there are many others in which no such resemblance exists; such is the fact with regard to the carcinomata and many histoid tumors, as the chondroma and osteoma particularly. The attempt, however partially successful, is nowhere paralleled in inflammatory processes.

As to the position to be accorded to such formations as angeioma, lymphangeioma, and dermoid cysts, Dr. Tyson fully agreed with Dr. Formad that, accurately speaking, they have no place among tumors. We continue to place them there rather from force of habit than for any scientific reason. Only in one particular—their correspondence with the etymological definition of "tumor," which means literally a "swelling"—do they comport with the correct notion of tumor.

Dr. F. P. HENRY said that he could not accept the theory of the inflammatory origin of tumors except in the general sense that they, as well as inflammatory products, are the result of perversions of nutrition. Its acceptance would necessitate a change in our views regarding the inflammatory process, compelling us to speak of a fibromatous, a lipomatous, an enchondromatous, and other hitherto unheard-of forms of inflammation.

While facts such as those mentioned by Dr. Formad furnished strong evidence in favor of the inflammatory origin of tumors, it should not be overlooked that there are at least equally strong facts opposed to this theory. Chief among these were the extraordinary frequency of inflammation and the comparatively extreme rarity of tumors. If a direct causal connection existed between inflammation and tumors, the latter would be more frequent. Dr. Formad had quoted the statement of a United States military surgeon that certain tribes of Indians enjoy an almost complete immunity from tumors, and there is no doubt whatever that the mode of life of these same Indians must render them peculiarly subject to inflammatory affections.

Prof. TYSON had referred, by way of illustrating a point in favor of the inflammatory theory, to the likeness presented by fibroma to a mature cicatrix, and that presented by sarcoma to granulation tissue. These are mere coincidences. To make this illustration of value it should be proved that fibroma originates as sarcoma, which, it is scarcely necessary to say, cannot be done.

Dr. HENRY acknowledged the pleasure and profit he had derived from Dr. Formad's pamphlet, and expressed his belief that it would be regarded as a standard work of reference by those interested in the subject of the etiology of tumors.

Dr. NANCREDE said that in reading Dr. Formad's valuable paper his attention had been arrested by certain statements from which he could not but feel compelled to dissent. According to the commonly-accepted view of the process of ossification, the discovery of islets of cartilage in adult bones is precisely what one would expect, especially when we know that traces of chondrogen are found in analyses of mature portions of the skeleton. Instead of being "misplaced germs," they are merely remains of the calcified fœtal cartilage situated at the points of mutual intersection of the periosteal ingrowths, which finally substitute all except traces of the fœtal structure. Even accepting Cornil and Ranvier's view,—which the speaker thought was, after all, reconcilable with the observations of other authors,—the "misplaced-germ" theory was utterly untenable. Dr. Nancrede thought that Dr. Formad had misunderstood his views as set forth in the quotation from his paper, as he would rank himself among the "inflammatory" as well as the "spontaneous" theorists. The speaker then gave a *résumé* of his own views, supporting them by certain positively ascertained facts as to the condition of the mammary gland at various ages, the effect of varying blood-supply to it and other organs, etc. He then stated that he considered Dr. Formad's views were incorrect as to "natural healing," or the reverse in its causative relation to morbid growths. Dr. Nancrede propounded the following: that when the irritant *and* the condition of the tissues were so related that the proliferation of cells was such as to keep pace with a sufficient blood-supply to admit of their development into tissue, normal healing occurred. If this proper relation failed to obtain, suppuration, caseation, or, perhaps, under certain circumstances, various morbid growths, would result. The speaker then mentioned certain facts which could be actually proven as to the relative atrophy of the connective tissue of the lip, the effect of continuous local irritation on the development of epithelioma, certain well-attested physiological facts, and contended that the missing links in his chain of, not reasoning, but facts, were practically demonstrable. He therefore repudiated Dr. Formad's dictum that all views but the inflammatory were mere theories,—"*that where nothing is proved there is nothing to disprove*,"—and quoted from the lecturer's paper on page 46, where he contended that the conclusions were *purely* theoretical and not logically warranted. Dr. Nancrede then endeavored to show that the failure of the connective-tissue bundles in rehabilitating themselves with their endothelial investment, if confirmed, and specially if demonstrated as a weakening of the connective-tissue barrier against epithelial ingrowths, was merely due to want of equilibrium between the blood-supply of the two tissues.

Dr. CHAS. K. MILLS said that he wished to put on record, in connection with Dr. Formad's valuable paper, a few notes on ten cases of brain tumor in which the post-mortem examinations had been made by him. These were cases in which close inquiries were made as to probable causation. In the majority of them, as will be seen, a history of traumatism was given. The notes were with reference to the history and the nature of the growths, as follows.

Case I.—Fall from high door-step, striking head. Fibroma.

Case II.—Wounded in the head by glancing bullet. Gumma.

Case III.—History of blows on the head and of syphilis. Gumma.

Case IV.—History of blows on the head and of syphilis. Gumma; also softening and abscess.

Case V.—Kicked by horse on the head. Fibroma.

Case VI.—Thrown from a horse and kicked on the head. Two growths: fibroma and gumma.

Case VII.—History of syphilis. Gumma.

Case VIII.—No history. Glioma.

Case IX.— " " "

Case X.— " " Carcinoma.

Dr. E. O. SHAKESPEARE felt that he could not allow the debate to close without expressing his high appreciation of the value, to the American physician, of the labor Dr. Formad had so successfully and learnedly performed in collating from the literature of the languages of the civilized world almost all of importance that has been thought and performed by distinguished men while attempting to elucidate the cause of tumors, and in classifying and abstracting, briefly, clearly, and forcibly, the various opinions of investigators. He had listened, much interested, while the lecturer with great ability and ingenuity proceeded to unfold and support his own belief concerning the etiology of tumors, and he had given close attention to the progress of the subsequent debate. He confessed that he had made no great study of the subject in question, and therefore did not feel entitled to entertain or express any very positive opinions; yet, during the course of the reading of the paper and of the discussion which had followed it, he had become more and more convinced of the necessity of exercising great caution in the acceptance of assumptions which may have little for their justification beyond a quasi-sequential order of appearance of certain phenomena, which is often, but by no means always, recognized in the history of tumors. He very much doubted the possibility, in the present state of our knowledge, of proving that inflammation either was or was not the essential cause of tumors. Certainly the lecturer, as well as other experienced investigators, must be credited when he affirms that in the majority of cases of tumor in which an adequate history has been recorded the growth has been preceded by a local inflammation or an injury. And yet even in these cases (supposing, for the sake of argument, there were no other) what right has any one to assume that the previous inflammation has acted as any other than a simple exciting cause? and who can rationally declare the tumor to be one of the natural terminations of the inflammation?

If the lecturer thinks to have discovered a general law concerning the etiology of tumors, let him and those who seem inclined to accept his hypothesis for one moment consider its application to syphilitic and tuberculous growths.

Dr. Mills has related, in the course of the debate, a number of cases of brain tumor, the histories of which showed that they followed a severe blow or other traumatism. In some instances the tumor proved to be sarcomatous, in others tuberculous, in others gummatous. Dr. Shakespeare referred to these particular cases because Dr. Mills had related them as perhaps offering some support to the hypothesis advanced in the paper, and had incidentally referred to him as personally cognizant of several of them. These cases are no more, perhaps no less, typical than others of that large class from which the essay has been made to deduce the general law enunciated this evening.

Will the lecturer take the position (seemingly absurd in the light of our present knowledge of syphilis and tuberculosis) that the tuberculous and the syphilitic tumors, no less than the sarcomata, are simply the natural terminations of an ordinary inflammation established by a traumatism? Or will he rather prefer to further curtail the list of tumors for the purposes of his theory, and erase the names of tubercle and gumma?

If the latter horn of the dilemma be elected, he would suggest the propriety of placing true carcinoma in a category very near to that of tubercle and gumma, for there are very many parallels and similarities in their clinical and pathological aspects.

He declared that he could see no cogent reason why some authors, in drawing the line of demarcation between abnormities which are and those which are not to be regarded as tumors, have placed upon the one side cancerous growths and upon the other side have ranged the permanent products of syphilis and tuberculosis. He did not recognize mere relative size as an adequate distinction between one morbid product which must, perforce, be a tumor, and another which must not be so classified. Carcinoma sometimes presents in its history the phase of miliary eruptions, and, on the other hand, tubercles oftentimes form a confluent tumor-mass of very considerable size and delimitation. All that is at present known of carcinomatosis and of tuberculosis would seem to warrant the

belief that in both there is frequently a strong hereditary predisposition. In both, from the locus of primary manifestation of disease the system may become infected by way of the lymphatics; in both the chain of lymphatic glands along the course of the lymph-vessels which lead from the primary growth may, and often does, form a cordon to prevent, at least for a time, the contamination of the general system; in both, when the morbid influence passes these natural barriers and reaches the circulating blood, metastases in various situations usually occur.

Notwithstanding the research and the observation and the ingenuity of the lecturer, Dr. Shakespeare thought that, as yet, we have no satisfactory reasons for attributing to an ordinary traumatic inflammation any agency in the development of a tumor beyond that of a simple exciting cause.

Dr. FORMAD, in closing the discussion, said, in reference to Dr. S. W. Gross's remarks, that he did not think it probable that tumors could arise from inoculation with tumor-juices unless the latter were the carriers of living tumor-particles. He believed that even in the three cases of apparent inoculation with tumor-juices just referred to by Dr. Gross there was no evidence at all that such small tumor-particles were not suspended in the juice and did not effect the transplantation of the new formations.

In reference to Dr. Tyson's remarks that the similarity between inflammatory products and tumors was limited to only a few instances, Dr. Formad maintained that this similarity was applicable to the majority of tumors, and, contrary to Dr. Tyson's view, was easily demonstrable,—*e.g.*, in carcinoma. Dr. Formad dwelt upon the gradual transition of inflamed skin into a cancroid, and of a catarrhal inflammation of the stomach or gall-bladder into cancerous growth,—the microscopic picture showing the direct merging of the primary inflammatory changes into cancer, and that it is impossible to point out where the one ends and the other commences.

Dr. Formad could not agree with Dr. Henry that there was necessary "a fibromatous, a lipomatous, an enchondromatous, and other unheard-of forms of inflammation" in accepting the view of an inflammatory origin of tumors. Dr. Formad thought that the ordinary process of interstitial and parenchymatous inflammation and the laws which govern the new formation of tissues are sufficient to explain the histogenesis of the various tumors. Fibroma, he thought, should be regarded as one of the products of a chronic interstitial inflammation; lipoma is nothing else than a uniform fatty infiltration of a fibroma, and a myxoma a mucoid degeneration of the latter. Before we can have adipose tissue we must have connective tissue; and probably all pathological mucous tissue has its origin in a mucoid degeneration of simple connective tisue. The direct transformation of fibrillar connective tissue into cartilage has been proven by several reliable observers. Thus the chondroma is formed. A parallel to this we have in the process of ossification and in the formation of osteoma, etc. Dr. Formad stated that he was acquainted with no real facts that could be brought forward against the view of an inflammatory origin of tumors.

Dr. Formad agreed with Dr. Nancrede that the quantity of the blood-supply conditioned the growth or the destruction of tissues, and determined frequently the variety of tumor-formation. He maintained, however, that only the inflammatory process was able to prepare a tissue anatomically or to predispose it to tumor-development. The destruction of the endothelial boundaries, the main causative factor, cannot be brought about by irregularity of blood-supply. Continuous hyperæmia, Dr. Formad thought, may bring on—for instance in the mammary gland—a homotopic adenoma, which is only a simple perverted epithelial hypertrophy, and not a true tumor. An injury to the elements of the skin, in the same mammary gland, will, under circumstances referred to, produce a surface epithelioma. An injury affecting the connective tissue of the gland will predispose to sarcoma (rapid effect) or to fibroma (slow effect), while the same cause operating upon the glandular elements proper (destroying the glandilemma) may induce a hard or a soft cancer.

In reference to Dr. Shakespeare's remarks, Dr. Formad stated that he did not mean to declare tumors to be a "natural" termination of inflammation, and that he was perfectly willing to call them a perverted termination of the inflammatory process, occurring only if the healing process is imperfect or retarded.

Tubercle and gumma Dr. Formad did not exclude from the category of tumors, and in them he thought that he had one of the best supports for an inflammatory origin of tumors. Dr. Formad did not think that the causes of the inflammation were here pertinent. In the case of tumor-formation it made no difference whether the operating cause of the inflammation was a specific poison, or a trauma, or anything else. The specific virus is not the cause of the gumma or tubercle, but is the cause of an inflammatory process, which in turn gives rise to the new formation. If the inflammatory changes are arrested, no tumors develop. The excellent series of cases of brain tumors referred to by Dr. Mills, Dr. Formad thought, supported admirably the view propounded.

Dr. Formad expressed his gratitude for the interest taken in the paper by the President and by the members of the Society, and for the many suggestive points ventilated by the discussion.

PHILADELPHIA ACADEMY OF SURGERY.

STATED MEETING OF DECEMBER 5, 1881.

DR. S. D. GROSS, President, in the Chair.

DR. MORTON presented a number of patients who were treated at the Orthopædic Hospital.

CONGENITAL EQUINO-VARUS—THE VARUS CURED BY STRETCHING THE EQUINUS BY DIVISION OF THE TENDO ACHILLIS.

Daniel S., æt. 7 weeks, was brought to the hospital January 22, 1880, with congenital equino-varus of the left foot of about the third degree. Treatment, manipulation and stretching.

February 12, 1880.—Varus much improved; equinus somewhat better.

July 1, 1880.—Improvement more marked.

January 20, 1881.—Child is just beginning to walk; has been wearing braces for three months; the varus is cured; equinus better, division of tendo Achillis will be necessary.

November 3, 1881.—Tendo Achillis divided.

November 30, 1881.—Cured.

RIGHT-ANGLE ANCHYLOSIS OF THE HIP—SUBCUTANEOUS OSTEOTOMY—CURED.

Marion T., æt. 10 years, was admitted to the hospital April 5, 1881, having suffered from coxalgia of right hip for four years. The trouble followed an injury, though there had never been an abscess, and all active symptoms had subsided.

An examination disclosed marked deformity about the hip, with an apparent anterior curvature of the spine. The hip was firmly anchylosed at a right angle, there was a shortening of the limb of two inches, with marked atrophy of limb and foot, measuring an inch and a half less in circumference of thigh and calf than the left limb.

Treatment.—On April 9 the neck of the right femur was divided subcutaneously with Adams saw, the limb was brought into a straight position and extension applied. Some febrile disturbance followed, which was relieved by mist. potass. citrat. The pain following the operation was subdued by morphia.

On May 17 was discharged much improved. At present (December 5), he is wearing a support for the limb when walking, and is in good health. The limb is about two inches short.

DOUBLE EQUINO-VARUS FROM INFANTILE PALSY.

Geo. W., æt. 19 years, was admitted to the hospital the 10th of November, 1881, with equino-varus. His family history was good. When two and a half years of age he suffered from infantile palsy of left limb.

There was no improvement for two or three years, when he became able to stand with assistance. On beginning to walk he found his foot turned over, growing worse till he was twelve years of age, when the inversion of the foot and elevation of the heel were very marked.

About this time he suffered from neuritis in the right limb, though there was no apparent cause: the ankle became swollen and inflamed. Treatment with cold and stimulating lotions relieved the condition in a week or ten days, so that he was able to walk with the aid of a cane. The limb atrophied, became weak and worse than the left. It grew more feeble for two years, when a brace was applied, since which time he has remained in the present condition, the feet being rigid, and presenting well-marked equino-varus with palsy of anterior muscles.

November 12, 1881.—Under ether the plantar fascia, the anterior tibials, and the tendo Achillis were divided in both feet, and the extensor tendons supplying the toes in the left.

Ordinary walking-shoes with lateral steel supports were applied November 29.

December 4, 1881.—Discharged walking well, with entire correction of the deformity.

Dr. Morton called especial attention to the atrophy which always exists after infantile palsies, and likewise in all cases of club-foot, even when the deformity has been entirely overcome, as well as the wasting and more or less permanent atrophy found in all limbs which have suffered from joint-inflammation, as seen in the case shown to the Society, which required subcutaneous osteotomy.

A CASE OF STRANGULATED SCROTAL HERNIA RELIEVED WITH THE KNIFE, AND FOLLOWED BY A PERMANENT CURE.

Prof. Gross presented the following.

I am indebted for the notes of this case to Dr. Wright, the resident physician of the Jefferson College Hospital, to whose assiduous care the patient is greatly indebted for his recovery.

Wm. Hickman, æt. 48, a hand in a paper-mill, was admitted to the Jefferson College Hospital September 30, 1881, on account of a scrotal hernia of twenty years' duration. On the morning of the day before he was admitted he fell through a hatchway, a distance of some twelve or fifteen feet. He noticed no inconvenience at first, and therefore continued at his work. In the afternoon of the same day, however, he began to have pain in the scrotum, and noticed that it was very much enlarged; he also had a sense of general uneasiness in the abdomen. He tried to reduce the tumor by taxis, but found it impossible. Notwithstanding this, he continued at his work until evening. When he reached his home, two physicians were sent for, who tried in vain for several hours to reduce it. He was now in a very critical condition. When admitted to the hospital, the tumor was almost as large as an adult head. His pulse was from 130 to 140 to the minute, hard and wiry. Temperature, 104°. Marked peritoni-

tis attended with stercoraceous vomiting was present. Immediate resort to the knife was deemed advisable, and he was accordingly taken before the class and etherized. The stricture was situated at the lower ring and was divided on the outside of the sac. No blood was lost, and he recovered in a short time from the effects of the anæsthetic. Three silver sutures were passed through the abdominal ring, and the wound closed with ordinary ligatures. The second morning after the operation the scrotum was found to be almost as large as it was before, and somewhat hot and tender to the touch. The parts were elevated and kept constantly wet with absorbent lint, but the enlargement did not disappear. The case progressed in this manner until, finally, fluctuation was discovered four weeks after the operation. The tumor was accordingly laid open and about eight ounces of pus let out. The parts were reduced at once almost to their normal size, and there has been no further trouble since. The patient has continued to improve, and is now as well as he ever was. The only noticeable thing in the scrotum is a plug of omentum extending into the groin and serving to close up the external ring, thus effectually protecting the parts against reprotrusion. The ends of the silver wire sutures were cut off close, with a view to their permanent retention. Care will be taken to protect the parts properly, for some time at least, with a suitable truss.

A CASE OF COMPOUND DISLOCATION OF THE SEMILUNAR BONE.

Dr. Gross exhibited the specimen and read the history of the case.

Twelve months ago a gentleman, aged about 32 years, in jumping from the platform of a car running at a speed of seventeen miles an hour, struck his hand upon a rock. He was unconscious for a few minutes, and was badly bruised in different parts of his body. A medical man who was called in soon after the occurrence of the accident found what he considered to be a compound dislocation of the wrist, a compound fracture of the radius and ulna, and two displaced carpal bones. Two days after this the gentleman came under the care of Dr. O'Hara, whom I met in consultation the next afternoon. As there was simply a wound half an inch in length on the inner side of the forearm anteriorly, and the limb was much swollen, it was deemed best not to disturb the parts by an examination, the more especially as there was a good deal of constitutional excitement, with progressive tendency. The treatment, local and constitutional, was rigidly antiphlogistic. The limb, wrapped up in a strong solution of lead and opium, was laid on an ice-bag, and the pain was allayed by hypodermic injections of morphia. Ten days later a loose bone was detected in the wound towards the radial side of the wrist, supposed to be a piece of the radius, but which proved afterwards to be the semilunar bone. The fever meanwhile ran very high and rapidly assumed a pyæmic type, attended with delirium, rigors, and elevated temperature. Extensive suppuration followed, and several sinuses formed, despite the most careful and assiduous attention, and for a time the case presented a formidable aspect. I must not forget to state that the shafts of the ulna and radius were fractured about four inches above the wrist-joint. There was no evidence at any time after the case came into our hands of a fracture of the inferior extremities of either of these bones.

The patient was ill for three months, and his health was so much shattered that he was advised to visit Florida, where, under the kind attention of Dr. Lopez, he gradually recovered, but not without some anchylosis of the wrist-joint and some defect in the functions of his hand, owing to the involvement of the sheaths of some of the flexor tendons.

O. H. ALLIS,
Recorder.

REVIEWS AND BOOK NOTICES.

THE SANITARY CARE AND TREATMENT OF CHILDREN AND THEIR DISEASES. Being a Series of Five Essays, by DRS. ELIZABETH GARRET ANDERSON, SAMUEL C. BUSEY, A. JACOBI, J. FORSYTH MEIGS, and J. LEWIS SMITH. Prepared by request of the Trustees of the Thomas Wilson Sanitarium of Baltimore, Md. Boston, Houghton, Mifflin & Co., 1881.

How to make haste slowly, how to wait in this age and land of hurry till thought and time have thoroughly matured their plans, seems a speciality of Baltimore trusteeship. In this case, among the valuable first-fruits of their patience we have this book of essays and plans, which will hold for years a foremost place as authority in the difficult question of how to take the best care of children.

The names of the writers are familiar to all. The nature of the book is clearly set forth in the introductory letter which elicited the essays. As showing that the care of children has attained somewhat of the dignity of scientific certainty, we would call attention to the fact that, diverse as are the surroundings of the writers, different as may be the climates they respectively inhabit, there is but little conflict of authority between them as to vitally important points. There is diversity enough; the writers seem to breathe a freer air than when confined to monograph, textbook, or didactic discourse, and it is the undesigned coincidences of all these practitioners who have distinguished themselves in life for their successful work in this department

of medicine which give their accumulated experience its weight and force.

The public are not yet sufficiently educated in the best methods of rearing children; and no better book can be found for the intelligent layman to read than this. The quiet certainty on fundamental points which in a treatise would seem dogmatic is here only incidental to the grand plan. The decalogue of child-rearing—the "thou shalt" and "thou shalt not"—comes to the reader with almost indisputable authority.

"How can Children in a City be kept Healthy?" is the title of the first essay, by Elizabeth Garret Anderson. The writer first inquires "what children most want in order to be in sound health." Her answer is, fresh air, suitable and abundant food, healthy parentage, protection from contact with sources of contagion and "all other influences which would interfere with the normal development and stability of the nervous system," and "to be the objects of a minute and all-watchful care." Pure air is considered the first and all-important requisite, and the author argues that if supplied with pure air children will thrive though food be insufficient, clothing scanty, and care wanting, but that all luxuries and comforts will fail if pure air be unattainable. Yet it is discouraging to read, "to children in towns, pure air is an impossible luxury." The author is sententious and graphic; her pages abound in wise sayings, the records of careful observation. She says, "Notice how much more robust the children of the careless class of mothers are when past early infancy than those of the careful." And "the best remedy for the indifferent quality of town air is to take a great deal of it in quantity." The causation of infantile diarrhœa is stated thus: "Everything points to the conclusion that the etiology of infantile diarrhœa is complex, and that high temperature is but one of its important factors, while impurity of air is another; probably the two combined induce some subtile changes in milk and other staples of children's food." She considers that the rules for children's diet should be "plain, minute, dogmatic," without attempt at physiological or chemical explanation, and that the chief hindrance to success lies in the "ignorance and unenlightened affection of the parents." The objectors to infant and primary schools as centres of infection are told that "poor children know no solitude," anyway. The writer pays considerable attention to the subject of the prevention of nervous disease. She credits American children with a tendency to "undue restlessness and irritability of nerves," and with precocity, the truth of which assertions—or aspersions—we have no means of proving. The essay is throughout instructive and suggestive.

Dr. Busey's essay is upon "The Mortality of Young Children, its Causes and Prevention." It is lengthy and statistical, and contains valuable work. The relations of infantile summer diarrhœa to the relative heat and humidity of the atmosphere are carefully studied. The questions of nutrition, infant digestion, artificial alimentation, are discussed in the most thorough manner. Space will not allow of an outline of its 136 pages, and we can only refer it to the reader as well worthy of study.

Dr. Jacobi's essay is upon "the Improvement of the Condition of the Poor and Sick Children; General Principles." In it are given the rules distributed by the New York Board of Health with such good results. Dr. Jacobi goes carefully into estimates and plans for the sanitarium, and gives his own views as to the proper way of feeding young children. The many statistics which he gives will also be found of great value.

Dr. John Forsyth Meigs contributes "Observations upon the Sanitary Care and Treatment of Children and their Diseases." This essay is exceedingly able and thoughtful; yet doctors disagree. While Dr. Anderson alludes to the fact that young children are as a rule always better suited by sea-air than any other as beyond question, we find Dr. Meigs speaking of the choice of a site for the proposed sanitarium, and recommending "an interior region of some altitude above the sea." Dr. Meigs quotes largely from the "Manual of Practical Hygiene" of Dr. Parkes, and discusses the question of the cost of a sanitarium and its general plan; he gives his views upon infant-feeding, which differ in some important particulars from those generally taught, as in the matter of beginning feeding at an early period. His personal observations on the question of how much milk a healthy woman secretes per day are of great interest, as also that part of the essay relating to the management of cholera infantum and diarrhœa, out-door exercise, and the proper clothing for children. The essay will be read with the interest which so high an authority can always command, even were it not written in that easy and graceful style in which the author notably excels. At the end will be found a suggested form of tract for distribution, which condenses his life-long experience into a convenient form.

The concluding essay is upon "the Causes of the Great Mortality of Young Children in Cities during the Summer Season, and the Hygienic Measures required for Prevention," by Dr. J. Lewis Smith. The ground occupied is similar to that of the other essayists; and, while we find these essays nominally upon the suggested subject of the sanitarium, the pages of each and every one teem with suggestions, hygienic, therapeutic, and dietetic, of the greatest value. There is little of value on these subjects to be learned from the best library, no matter how extensive, which cannot be found on the pages of this book of essays.

E. W. W.

FROZEN SECTIONS OF A CHILD. By THOS. DWIGHT, M.D. New York, Wm. Wood & Co.

This book does not belong to the yearly medical library of the publishers, and therefore has not been written to order to fill up a gap, and is presumably published because its author thinks he has something fresh to say or to show. Dr. Dwight is instructor in Harvard University, and the sections were originally made for teaching purposes. There are fifteen plates in all, boldly and strongly drawn, with excellent descriptions, or rather discussions, of them in the text, which is bound with the plates in such a way that the pictorial representation is immediately followed by the printed thought.

GLEANINGS FROM EXCHANGES.

PERMANGANATE OF POTASSIUM IN SNAKE-BITES.—M. de Lacerda (*Medical Times and Gazette*, October 29, 1881) has lately discovered a fact of considerable scientific and practical importance, which he has communicated in a note to the Paris Academy, namely, that permanganate of potassium counteracts very effectively the poison of snakes. In a first series of experiments, a water solution of the poison was injected into the cellular tissue of dogs, under the legs, and its usual effects were large swellings, with abscesses, loss of substance, and destruction of tissues. But when an equal quantity of filtered (one per cent.) solution of permanganate of potassium was injected one or two minutes after the poison, these local injuries were quite obviated; there was merely a slight swelling where the syringe had entered. Next, introduction into the veins was tried, and the permanganate again succeeded admirably. In only two cases out of more than thirty was there failure, and this is attributed to the animals experimented on being very young and weak, and badly fed; also to the antidote being administered at too long an interval after the poison, when the heart was already tending to stop. In one series of cases the permanganate solution was introduced half a minute after the solution of venom, and the animal operated on showed no derangement beyond a very transient agitation, and acceleration of the heart's action for a few minutes. In another series, the characteristic troubles caused by the poison were allowed to manifest themselves (dilatation of the pupil, quick breathing and heart-action, contractions, etc.) before the antidote was applied. In two or three minutes, sometimes five, the troubles disappeared; a slight general prostration followed for fifteen to twenty-five minutes; after which the animal would walk, and even run about, and resume its normal aspect. Other dogs poisoned similarly, but not receiving the antidote, died more or less quickly.

NITRATE OF SILVER IN SCIATICA.—Subcutaneous injection of nitrate of silver has been strongly recommended and frequently employed by M. Le Dentu, of the St. Louis Hospital, in the treatment of sciatica. The following is a case that succeeded in the hands of Dr. Greslon. A woman æt. 53, of a rheumatic diathesis, suffered from all the classic symptoms of sciatica in the left leg. The whole member, from the external malleolus to the sacro-sciatic notch, was the seat of lancinating pain, which lasted a month. All the ordinary means being used, but without effect, the patient consented to the subcutaneous injection of the agent in question. Five drops of a solution (1 in 4) were injected in the upper and posterior portion of the thigh, at a point corresponding to the exit of the sciatic nerve. This injection was immediately followed by great pain, and an abscess formed, giving exit to a teaspoonful of pus. Three or four days afterwards the patient was able to get up and walk about, all pain having left the leg. However, the point corresponding to the sciatic notch remained as sensitive as before, and another injection was practised, which was not followed by an abscess, and in a week the cure was complete. This case is not only interesting from the rapidity of the cure, but also from the fact that the subject was rheumatic, and consequently was unfavorable as to prognosis.—*Medical Press and Circular*, October 12, 1881.

TREATMENT OF PURULENT ENDOMETRITIS WITH ULCERATION OF THE CERVIX.—Dr. Chéron remarks that patients suffering from a purulent discharge, the result of endometritis, with or without ulceration, are frequently unable to bear injections of such substances as coal-tar, which are particularly apt to dry the secretion. In such cases Dr. Chéron finds it useful to employ the following solution of tannic acid in glycerin:

Tannic acid, 60 grams.
Sydenham's laudanum, 10 grams.
Neutral glycerin, 350 grams.

Dissolve the tannic acid in the glycerin by means of heat, without using water, then filter and add the laudanum, viz., one or two dessert-spoonfuls to be added to a litre of warm water; injections to be made morning and evening. The effect of the injections is to cause a rapid diminution of the purulent secretion. The pruritus and irritation of the external parts disappear, whilst the sensations of weight and pain are less felt after a few days. If there be no ulceration, the dose of laudanum may be increased to twenty, or even to thirty, grams, without inconvenience. —*Le Progrès Médical.*

OSSEOUS LESIONS IN HEMIPLEGIC SUBJECTS.—Before the Société Médicale des Hôpitaux, M. Debove recently made a few observations on the osseous lesions met with in hemiplegic subjects. As surgeon of the Bicêtre Hospital, he had observed several

fractures in paralytic subjects, and he soon remarked that the fracture was always on the side of the hemiplegia. There was reason to think that the bones of the affected side had undergone some alteration that rendered them more friable. At the autopsy of an hemiplegic who had fractured the humerus, M. Debove was able to observe that not only had the fractured bone undergone this alteration, but all the bones of that side were affected. In comparing the two sides he found that the humerus of the side affected with paralysis was lighter than the opposite. On making a transverse section of the bone, the medullary canal was larger than usual, and the substance of the diaphysis was less compact. The Haversian canals were greatly dilated, and the bone was porous. There was evidence also of fatty degeneration. These lesions were sufficient to explain the relative frequency of fractures in hemiplegics. M. Debove further observed that these fractures united very rapidly.—*Medical Press and Circular.*

Drainage of the Pericardium.—A case probably unique in the annals of paracentesis has been recorded by Rosenstein of Leyden. A child, aged 10 years, suffering from pericardial effusion, presented such a degree of interference with circulation and respiration that an aspirator-needle was passed into the fourth intercostal space near the sternum, and 620 cubic centimetres of liquid were withdrawn. Left-sided pleural effusion soon followed, and 1100 cubic centimetres of liquid were evacuated. The cardiac symptoms increased, and necessitated a second puncture of the pericardium; 120 cubic centimetres of purulent liquid were withdrawn. A relapse occurring, a larger opening was made (an inch and a half long) in the fourth intercostal space. The soft parts were divided layer by layer under strict antiseptic precautions. When the pericardial cavity was reached, a large quantity of pus escaped. Two drainage-tubes were inserted. The operation was followed by an immediate return of the circulation and respiration to normal conditions. An incision into the pleura, however, also became necessary. At the end of four months of treatment the patient left the hospital in good condition. There was no pyrexia or œdema of the skin in the præcordial region to indicate the purulent nature of the effusion.—*Lancet*, October 15, 1881.

An Agreeable Addition to Tincture of the Chloride of Iron.—Dr. Reed, of Montreal, says that the addition of citrate of potassium to tincture of iron will disguise its, to many, unpleasant taste. For a tablespoonful dose containing ten minims the prescription may be: Tinct. ferri chlor., f℥ij; potass. citrat., ℥j; syr. limonis, ℥iss; aquæ ad f℥vj. Another advantage of the mixture is that astringent tinctures, as bark, gentian, etc., may be added without decomposition.

Ablation of Fibroid Tumors of the Uterus.—At the Académie de Médecine M. Guéniot communicated some observations on the different methods employed in the ablation of fibroid tumors of the uterus. He would give the preference to excision by the aid of the constrictor or *serre-nœud*, on account of its simplicity and its security. He considered it even superior to Chassaignac's écraseur, in that it can be applied with greater facility, and that it can attain polypi situated on the fundus of the uterus without necessitating a previous traction on the organ. The operation is as bloodless as when the écraseur is employed, and the pedicle is cleanly cut across.

Arseniate of Soda in Psoriasis.—Dr. Guibout prescribes 1 centigramme of the arseniate with 1 gramme 60 centigrammes of the extract of gentian, dividing into ten pills, of which from two to three are given at each of the three meals; or, instead of the pills, from 1 to 2 tablespoonfuls of the arseniate, 10 centigrammes in 500 grammes of distilled water, may be taken at each meal. The arseniate is to be continued in some forms of the disease for from six to twelve months after the disappearance of the eruption. Repeated purgatives must be given; and if the patient is robust, alkaline preparations; while if he is weak and anæmic, tonics and preparations of iron must be resorted to. As an external application, 10 to 15 parts of pyrogallic acid to 100 of lard may be employed, soapy baths being used every two or three days for cleansing the skin. Juniper oil (*l'huile de cade*) used in frictions twice a day may be substituted for the baths. The treatment should be completed with alkaline baths.—*Union Médicale*, October 20, 1881.

Formula for Bromidia.—

℞ Potassium bromide, 4 ounces;
Chloral hydrate, 4 ounces;
Ext. hyoscyamus, 16 grains;
Ext. cannabis indica, 16 grains;
Alcohol, 2 ounces;
Water, q. s. to make one pint.

Kelner's Compendium.

MISCELLANY.

Prize-Rewards offered by the Danish Society for the Protection of Animals.—The Danish Society for the Protection of Animals (under patronage of His Majesty the King of Denmark) offers two prizes, of two thousand and one thousand francs respectively, for the best and second best scientific essay on that part of the vivisection question which concerns the possibility of replacing *living* by *recently-killed animals* during physiological investigations, and sufficiently indicates not before known cases in which such a substitution of dead material may be applicable.

We refer specially to a well-known declara-

tion of Prof. M. Schiff, that "under certain circumstances the functions of life may be studied in recently-killed animals."

In these essays the possibility and desirability of replacing painful experiments on animals by some *other methods of research* may also be a subject of inquiry.

The essays may be written in the Danish, Swedish, English, French, or German language. They must be clearly and legibly written, signed with a motto, which is also to be placed on an accompanying sealed envelope containing the name and address of the writer. These are to be forwarded before the 1st of September, 1882, to His Excellency Mr. A. de Haxthausen, President of the Danish Society for the Protection of Animals, at the office of the Society, Copenhagen.

The board of directors will secure scientific assistance in awarding the prizes. In the event of none of the essays possessing sufficient merit to warrant a prize, smaller rewards will be given to those competitors whose essays bear evidence of ability as well as sympathy with the object of the Society, which reserves all rights of publication.

Our Society is only too well aware that the claims of humanity are not to be satisfied by these means as extensively as it should wish. It will, however, feel itself richly rewarded if its efforts result in diminishing the number of experiments in which animals are subjected to great and lingering agony. In this earnest hope, we respectfully request all humanely-feeling scientists of every country in the world kindly to comply with our challenge.

THE BOARD OF DIRECTORS.

COPENHAGEN, July, 1881.

THE OVERPLUS IN THE PROFESSION.—That the profession is overloaded, not only in this country, but also in England, is evidenced by the number in England who openly advertise medicine and advice at sums varying from threepence to one shilling per week, and especially by the pressure for government positions in the English dependencies. About a month since, an appointment of medical officer to the Wynaad mining district was advertised, at a salary of seven hundred pounds per annum, —not a very lucrative post for India; yet seventy candidates applied, and were ready to risk the unhealthy climate of that locality for this emolument.

RESORCIN IN AURAL SURGERY.—Dr. Rossi, of Rome, communicates to the *Archives of Otology* a note on the use of resorcin in aural practice. This substance, a parabioxy benzole, discovered by Hlaswitz and Barth in 1864, was introduced into medical practice by Andeer in 1877. Dr. Rossi has used it in more than two hundred cases of purulent middle-ear inflammation, and claims that *no* remedy at his disposal has given such substantial results in this obstinate affection as resorcin. He has used it *pure*, or in aqueous or alcoholic solution, 4 in 100. He promises further records of results.

PENNIES OUT OF PLACE.—A coroner's inquest was held lately in London on the body of a child, three years of age, whose death had been caused by swallowing a coin in some sweetmeats. From the evidence adduced, it appears that in the neighborhood of Clerkenwell packets of sweets are sold in which coins are placed, being baked in the centre. The deceased child got a farthing in the centre of the sweet she purchased, and she swallowed it, with the result of causing inflammation, peritonitis, and death. A number of deaths have taken place in this locality from the same cause.

ERASMUS WILSON, the dermatologist, recently elected President of the Royal College of Surgeons, has received the honor of knighthood and the distinction of a cartoon in the London *Vanity Fair* and *Punch*.

DR. J. S. BILLINGS and DR. H. J. BIGELOW, of Boston, have been elected honorary members of the London Clinical Society.

NOTES AND QUERIES.

THE PATHOLOGICAL SOCIETY OF PHILADELPHIA is about issuing its Transactions for 1880–81. Desiring to give the valuable matter in the volumes already published a wider circulation, the committee has decided to dispose of single volumes at sixty cents, including postage. Whole sets will be furnished at fifty cents a volume.

OFFICIAL LIST

OF CHANGES OF STATIONS AND DUTIES OF OFFICERS OF THE MEDICAL DEPARTMENT U.S. ARMY FROM DECEMBER 11 TO DECEMBER 24, 1881.

ALEXANDER, R. H., MAJOR AND SURGEON.—The leave of absence granted him in S. O. 215, September 19, 1881, from A. G. O., is extended one month. S. O. 285, A. G. O., December 17, 1881.

MCCLELLAN, ELY, MAJOR AND SURGEON.—Relieved from duty at Fort McHenry, Md., to proceed to Fort Trumbull, Conn., and relieve Assistant-Surgeon W. H. King. S. O. 224, Department of the East, December 15, 1881.

KING, W. H., CAPTAIN AND ASSISTANT-SURGEON.—When relieved by Surgeon McClellan, to repair to Fort McHenry, Md., and report for duty at that post. S. O. 224, c. s., Department of the East.

HOFF, J. V. R., CAPTAIN AND ASSISTANT-SURGEON.—Granted leave of absence for one month. S. O. 222, Department of the East, December 12, 1881.

PERLEY, H. O., CAPTAIN AND ASSISTANT-SURGEON.—Having reported in person at these Headquarters, will report to the commanding officer, Fort Columbus, N.Y.H., for duty as assistant to the post-surgeon and attending surgeon at these Headquarters. S. O. 224, c. s., Department of the East.

GORGAS, W. C., FIRST-LIEUTENANT AND ASSISTANT-SURGEON.—Granted leave of absence for one month, with permission to apply for an extension of one month. S. O. 150, Department of Texas, December 3, 1881.

RAYMOND, HENRY J., FIRST-LIEUTENANT AND ASSISTANT-SURGEON.—Relieved temporarily from duty at Alcatraz Island, Cal., and to report to the commanding officer of the Presidio of San Francisco for duty at that post. S. O. 214, Military Division of the Pacific and Department of California, December 9, 1881.

MADDOX, THOS. J. C., FIRST-LIEUTENANT AND ASSISTANT-SURGEON.—Having completed the duties assigned him under S. O. 248, November 3, 1881, from A. G. O., will report in person to the Surgeon-General U.S. Army. S. O. 238, A. G. O., December 15, 1881.